

The Large Deployable Reflector (LDR) Report of the Science Coordination Group

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Jet Propulsion Laboratory
California Institute of Technology
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ABSTRACT

The Large Deployable Reflector (LDR) is a telescope designed to carry out high-angular resolution, high-sensitivity observations at far-infrared and submillimeter wavelengths. NASA has carried out several system studies of the LDR and has held two workshops, in June 1982 and March 1985, at Asilomar, California. A Science Coordination Group (SCG) was established by NASA in September 1983 to review the goals of the LDR, to provide definitive design criteria, and to interact closely with the technical studies. This report of the SCG activities complements the several study reports and the Asilomar workshop reports. Some of the major issues addressed in the SCG report are discussed below.

The scientific rationale for the LDR is discussed in light of the recent Infrared Astronomical Satellite (IRAS) and Kuiper Airborne Observatory (KAO) results and the several new ground-based observatories planned for the late 1980s. The importance of high-sensitivity and high-angular-resolution observations from space in the submillimeter region is stressed.

The scientific and technical problems of using the LDR in a "light bucket" mode at $\lesssim 5 \mu\text{m}$ and in designing the LDR as an unfilled aperture with subarc-second resolution are also discussed.

The need for an aperture as large as 20 m is established, along with the requirements of beam-shape stability, spatial chopping, thermal control, and surface figure stability. The instrument complement required to cover the wavelength-spectral resolution region of interest to the LDR is defined.

FOREWORD

The Science Coordination Group (SCG) was appointed by NASA Headquarters Code EZ to advise and coordinate the Large Deployable Reflector (LDR) study activities. The group was formed in September 1983. The SCG activities peaked with the Asilomar II LDR workshop in March 1985. This report represents the work of the SCG through May 1986.

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CONTENTS

EXECUTIVE SUMMARY.	xi
I. INTRODUCTION.	1-1
A. DESCRIPTION OF THE LDR	1-1
B. COMPARISON OF THE LDR WITH CURRENT AND FUTURE INFRARED AND SUBMILLIMETER TELESCOPES	1-6
C. HISTORY OF THE LDR	1-8
D. RECENT LDR ISSUES	1-11
1. NASA Technical Review	1-11
2. Space Station Assembly of the LDR	1-12
3. SCG Focal-Plane and Instruments Study	1-12
4. The Light Bucket Mode	1-15
5. The Interferometer Mode	1-15
II. SCIENTIFIC RATIONALE	2-1
A. INTRODUCTION	2-1
B. EARLY STAGES OF STAR FORMATION	2-5
C. PROTOSTARS, OUTFLOWS, AND PLANET FORMATION	2-9
1. Formation of Protostars	2-9
2. Protostars and Outflows	2-11
3. Planetary Formation	2-13
D. EXTRAGALACTIC ASTRONOMY	2-13
1. CI	2-16
2. OI, OIII	2-16
3. CII	2-16

III. SYSTEM REQUIREMENTS AND TRADES	3-1
A. APERTURE VS SCIENCE	3-1
B. THE INTERFEROMETER OPTION	3-4
1. The Array	3-5
2. The Hybrid	3-6
3. Interferometer Science Discussion	3-8
C. THE LIGHT BUCKET	3-11
D. INSTRUMENTS AND CRYOGENS	3-12
1. Instruments	3-12
2. Cryogenics	3-13
3. Generic Electronics	3-16
E. ANCILLARY REQUIREMENTS	3-17
1. Beam Stability	3-17
2. Space Station and LDR	3-19
3. Telemetry	3-22
4. Chopping, Thermal Control, and Figure Control	3-22
5. LDR Orbital Environment	3-25
IV. DEVELOPMENT PROGRAM LEADING TO THE LDR	4-1
A. REFLECTOR PANEL DEVELOPMENT	4-1
B. STRUCTURES AND DEPLOYMENT DEVELOPMENT	4-3
C. INSTRUMENT DEVELOPMENT	4-5
D. PRECURSORS	4-5
1. Precursors of Other Premiere Instruments	4-5
2. The State of Technological Readiness for the LDR	4-10
3. The State of Scientific Readiness for the LDR	4-10
4. The Scale of the LDR Precursor	4-11

E.	CONCLUSIONS	4-12
F.	THEORETICAL MODELING	4-13
G.	LABORATORY ASTROPHYSICS	4-14
REFERENCES		5-1

APPENDIXES

A.	FURTHER SCIENCE JUSTIFICATION ARGUMENTS	A-1
B.	TYPICAL OBSERVATIONS	B-1
C.	THEORY OF OFFSETS CAUSED BY RANDOM DEFECTS AND DUST	C-1

Figures

1-1.	Artist's concept of a Shuttle-deployed LDR	1-2
1-2.	Atmospheric transmission from 1 μ m to 1 mm at an altitude of 4.2 km, representative of the best from a mountaintop observatory	1-3
1-3.	Summary of the LDR science objectives	1-10
1-4.	Provisional configuration of focal-plane instrumentation . . .	1-14
2-1.	Range of sizes of astronomical objects plotted vs range of their typical distances	2-3
2-2.	Wavelengths and excitation environments of selected astrophysical lines	2-4
2-3.	Spectrum of Orion A molecular cloud between 208 and 262 GHz	2-6
2-4.	Collapse of an interstellar cloud to form a stellar system . .	2-8
2-5.	NH ₃ J _K = 1 ₀ - 0 ₀ transition observed by Keene, Blake, and Phillips (1983) in OMC-1	2-10
2-6.	KA0 observation of the 4 ₁₄ -3 ₂₁ transition at 790 μ m by Phillips et al. (1980) in the Orion Molecular Cloud core . .	2-12
2-7.	M31 (IRAS)	2-14
2-8.	OIII line emission from M82	2-17
2-9.	CII and CO line emission from M82	2-18

3-1. Range of resolution and frequency covered by the LDR strawman instruments	3-15
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Tables

2-1. Detected Species and Line Survey Parameters	2-7
3-1. LDR Strawman Instrument Complement	3-14
3-2. Major Advantages and Disadvantages of LDR Options	3-20
3-3. Annual Average Efficiencies	3-27
4-1. LDR Instrument Development Requirements	4-6

The Large Deployable Reflector (LDR) is a telescope designed to carry out high-angular-resolution, high-sensitivity observations at far-infrared and submillimeter wavelengths. Since the design of such a telescope represents a major scientific and engineering challenge, NASA, with participation of the scientific and industrial communities, has carried out studies aimed at defining design approaches for the reflectors, structure, controls, and sensitive instruments required for the LDR. The results of the first major workshop at Asilomar, "Large Deployable Reflector Science and Technology Workshop," were published in 1982. Following that initial effort, NASA contracted with Eastman Kodak and McDonnell Douglas for more detailed technical studies of the principal systems necessary to enable the LDR. A separate study was also carried out by JPL. A Science Coordination Group (SCG), comprising representatives from universities and NASA centers, was established to review the scientific goals of the LDR, to provide definitive design criteria for the LDR, and to work closely with the technical studies. The conclusions of these studies are reported in the Studies Reports. This report of the SCG complements the technical studies reports by describing the tasks undertaken by and the findings of the SCG. The following are some of the major issues examined and brief summaries of a number of associated conclusions discussed in greater detail later:

- (1) Scientific rationale in light of the Infrared Astronomical Satellite (IRAS), Kuiper Airborne Observatory (KAO), and other discoveries since the Asilomar I LDR Science Report

These scientific advances emphasize the urgency, importance, and potential of constructing a high-sensitivity, high-angular-resolution submillimeter telescope.

- (2) Scientific benefits and technical problems of extending the imaging range of the LDR to wavelengths as short as 3 μm by designing the telescope as a "light bucket"

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The light bucket mode should be considered a desirable goal, but not a requirement of the LDR; the fundamental goal is a large-aperture telescope, diffraction limited at 30 μm and operating in the atmosphere opaque region from 30 μm to 1 mm.

- (3) Scientific benefits and technical problems of designing the LDR as an unfilled aperture telescope capable of achieving subarcsecond resolution

The LDR baseline should remain a large single-aperture telescope without precluding the eventual possibility of the inclusion of the LDR into a space-based interferometer.

- (4) Scientific requirements on the aperture size

A 20-m aperture should remain the baseline; a smaller telescope will have severe sensitivity and confusion problems for many important scientific problems.

- (5) Instrument complement required for scientific goals, instrument technology issues, and instrument cooling requirements

Eight generic types of instruments are required to provide the range of imaging, spectral resolution, and wavelength coverage to meet the high-priority LDR scientific goals. An initial complement should include at least four instruments. Instrument changeout is essential to the LDR concept. A variety of instrument technology, especially in the area of heterodyne receivers, requires significant technical development and consequently requires extensive and early investment. Development of refrigeration technology is critical to achieving the LDR goals.

- (6) Beam stability requirements

The beam (point spread function) should be sufficiently stable to provide a dynamic range of 250:1 required for both continuum and spectral line modes.

(7) Chopping, thermal control, and figure stability

Thermal background stability (1 part in 10^8) is critical for submillimeter continuum measurements and may be difficult to achieve. Thermal background modeling and experimental confirmation of several strawman LDR designs should be a high-priority engineering goal at this time. In case the cost of a thermally stable LDR proves to be prohibitive, the SCG recommends study of lower cost alternative technologies suitable for meeting the spectral line requirements.

(8) Nature of the LDR surface

Lightweight, actively controlled mirror panels are essential for reducing the LDR weight and cost. A continuing program of lightweight glass and composite mirror development and evaluation is essential. A flight demonstration may be important to validate the design of the surface elements and surface control system and to space qualify advanced heterodyne receivers. Other SCG responses have been incorporated into the final technical reports.

(9) Relation of the LDR to the Space Station

Space servicing is essential for cryogen replenishment and instrument changeout.

All of the technical studies identified the thermal stability and chopping requirements for the LDR as control design concerns. The SCG believes that future studies might wish to consider the possibility of advancing the spectral line and continuum requirements and explaining the implications for design costs of more specialized instruments.

SECTION I

INTRODUCTION

A. DESCRIPTION OF THE LDR

The Large Deployable Reflector (LDR) (see Figure 1-1) will be an orbiting telescope of 20-m diameter, constructed by novel technologies. The deployment of this large high-precision antenna in space will provide scientists with a powerful new tool for studies of far-infrared and submillimeter radiation. The LDR will study newly forming stars and planetary systems with unprecedented sensitivity and angular resolution. The LDR will also detect the ancient redshifted signals from forming galaxies, galaxy clusters, and pregalactic gas located at the very edge of the universe.

Over the past two decades, exploratory satellites and, more recently, fully equipped space observatories have permitted the study of several regions of the electromagnetic spectrum not available from the surface of the Earth. In combination with optical and radio information, obtained by the concerted efforts of generations of astronomers working from the ground, the new data have helped to reveal a picture of the universe that reflects the full riches of its varied and beautiful phenomena. The universe presents a different face in each energy domain. The appearance of the X-ray sky (sensitive to emission from million-degree gas) is vastly different from the familiar optical sky (sensitive primarily to radiation from bodies of temperatures 5000 to 50,000 K). The extensive wavelength region between 30 μm and 1 mm, known as the far-infrared (30-200 μm) and the submillimeter (200 μm to 1 mm), is blocked by the Earth's atmosphere except in narrow, translucent windows (Figure 1-2). Since the equilibrium temperatures for most of the gas and dust in interstellar space range from 10 to 1000 K, this material radiates primarily at infrared wavelengths, making this wavelength region crucial for studying cool material associated with a great variety of astrophysical objects. Observations of this cool material or of highly redshifted hotter objects carry critical information regarding the assembly of galaxies and stars. The breadth and importance of this wavelength region to the understanding of the universe was emphasized by the enormous success of the Infrared Astronomical

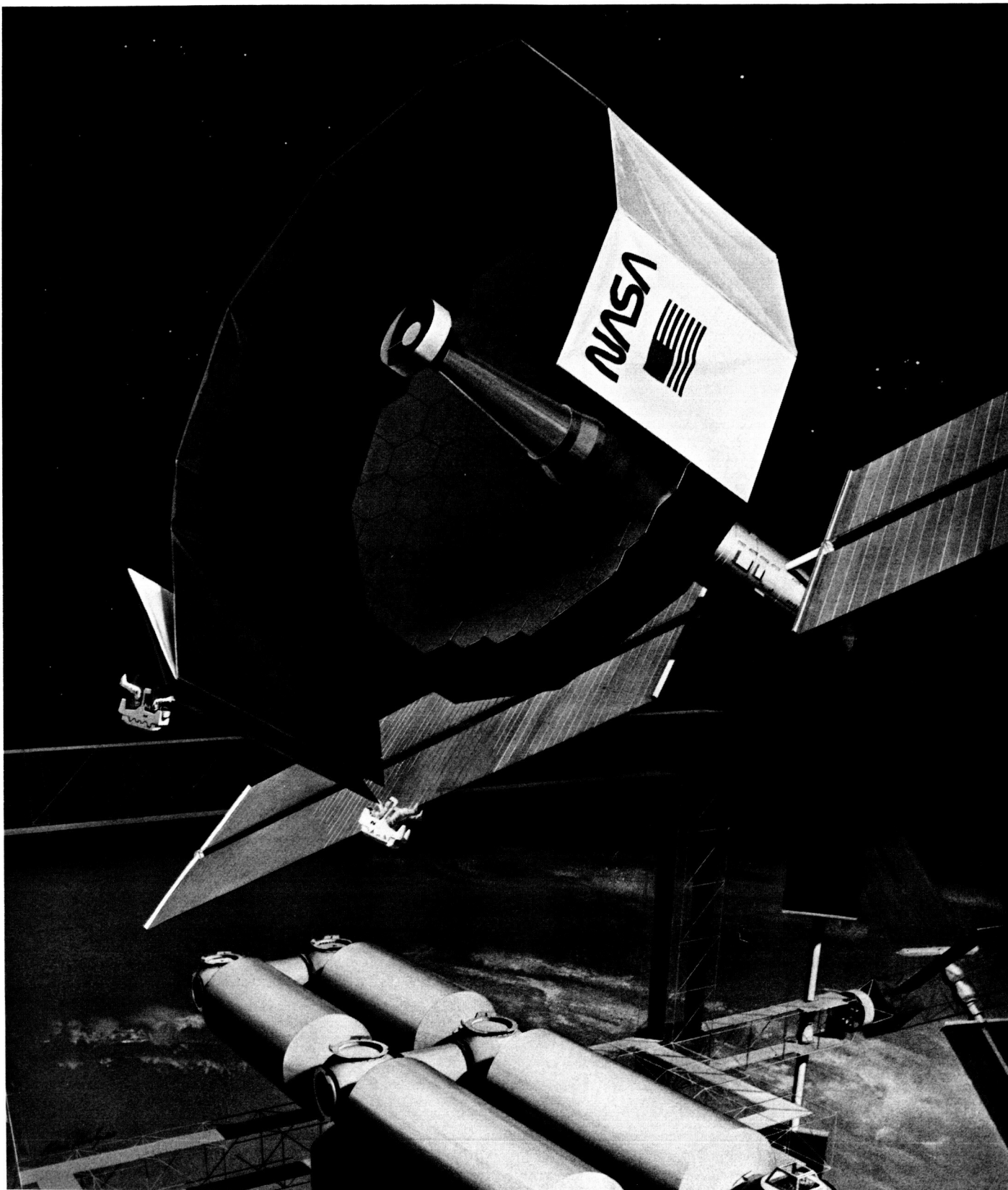


Figure 1-1. Artist's concept of a Shuttle-deployed LDR

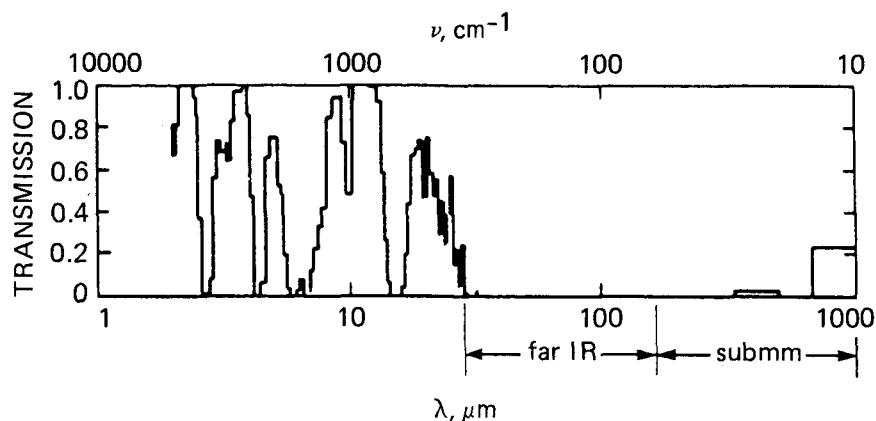


Figure 1-2. Atmospheric transmission from 1 μm to 1 mm at an altitude of 4.2 km, representative of the best from a mountaintop observatory. The bandpass varies from 5 to 100 cm^{-1} to keep the resolution on the figure roughly constant. Most of the absorption over this band is due to water vapor; a column density of 2.4 precipitable millimeters is assumed.

Satellite (IRAS), which opened the era of infrared astronomy from space by surveying almost the entire sky at 12, 25, 60, and 100 μm . IRAS returned a wealth of data revealing unknown characteristics of interstellar matter, star formation process, disks of solid matter around nearby stars, infrared-bright star-burst galaxies, and distant quasars. While IRAS provided a broad overview of the infrared sky, it also raised many new questions that require observations with great spatial and spectral resolutions for their solution. IRAS revealed the richness of the far-infrared and submillimeter sky--but at an angular resolution of ~ 60 arcsec: 60 times less than that of the Palomar Observatory sky survey. Such relatively poor angular resolution is insufficient to image forming galaxies or solar systems. The small aperture of IRAS precluded all but the most modest spectroscopic survey of the brightest far-infrared sources. To image the far-infrared sky at an angular resolution comparable to ground-based telescopes requires an aperture ~ 30 times that of IRAS: 20 m. An aperture of this size is also required to obtain spectra of sufficient resolution and signal/noise to chart the chemical components, temperatures, densities, and kinematics of far-infrared sources. An LDR of this design will provide unique insight into

- (1) The formation of stars and planetary systems. Stars and solar systems begin their lives in the cold depths of optically opaque clouds of dust, atoms, and molecules. At far-infrared, submillimeter, and radio wavelengths, these clouds are transparent, and feeble radiation within them can only be seen and studied by telescopes of sufficient size and sensitivity. A large instrument such as the LDR, operating in space at far-infrared and submillimeter wavelengths, provides a viable tool for studying the physical and chemical processes which accompany the coalescence of this material into protostellar units. With the LDR we will, for the first time, be able to witness the growth of sun-like stars and to learn whether the formation of planetary systems is a necessary or common result of stellar births.
- (2) The nature of "infrared galaxies." IRAS also discovered distant galaxies that emit more than 99 percent of their energy in the far-infrared. These extremely luminous objects may be powered by bursts of star formation or by more exotic energy sources such as galaxy collisions or black holes. However, IRAS was unable to distinguish the origin of this radiation--whether from the disk or a central nuclear engine, the LDR will probe the structure and dynamics of these visually obscured galaxies and provide critical information on the location of the infrared engine(s) and the physical conditions that exist within and around them.
- (3) The birth of galaxies and galaxy clusters. The LDR will permit us to observe the fluctuations in the cosmic background radiation. These highly redshifted signals from the first matter to decouple from the "primordial soup" hold the key to understanding how the universe achieved its current state and contain information (redshift, chemical composition, gas kinematics, and role of dust) concerning the nature and evolution of galaxies at the epoch of formation. Also, the evolutionary fate of the universe may be learned from the study of the cosmic background radiation as it interacts with the hot, diffuse material which pervades rich clusters of galaxies. From LDR temperature measurements made with

a precision of one part in a million, we can determine whether matter in the universe will continue to disperse or eventually slow down and contract to a singularity, perhaps to recommence the evolutionary cycle of the universe.

The LDR will be a major component in NASA's long-term commitment to place in space permanent, orbiting astronomical observatories that are sensitive to radiation over the full range of the electromagnetic spectrum. During this decade, NASA expects to launch the Space Telescope (ST), the first large, permanent optical/ultraviolet orbiting observatory. The ST should provide the necessary observations to establish an accurate distance scale for the universe and to chart the evolutionary history of galaxies over the past 90 percent of the age of the universe.

The Gamma Ray Observatory (GRO), now under construction, will study energy transformations in critically important processes, such as cosmic explosions, acceleration and interactions of high-energy particles, gravitational accretion by superdense objects, nucleosynthesis in stars, and matter-antimatter annihilation.

Later, the Advanced X-Ray Astrophysics Facility (AXAF) will permit the panoply of high-energy phenomena in the universe to be studied. The hot gas pervading clusters of galaxies, the plasma entering supermassive black holes in galaxy centers, and the signals from starquakes on superdense stars will all be observable with AXAF.

SIRTF will probe the infrared sky with very high sensitivity to continuum sources in the thermal infrared, providing an essential high-angular-resolution complement to the IRAS survey at wavelengths shorter than 30 μm . SIRTF should provide important new insight into the population of star-forming regions, dark matter in the universe, and the evolutionary behavior of infrared-bright galaxies out to the edge of the universe.

Among this complement of extraordinary observatories, the LDR represents a significant departure in design and philosophy. Because of its immense size, the LDR cannot be launched with a single vehicle. Instead, the LDR will

be the first astronomical observatory to be erected and assembled in space, a distinction that brings with it major challenges to current technology. At the same time, achieving LDR objectives will provide invaluable experience in the art of constructing high-precision, large space structures. The benefit of bringing this new art into the service of astronomy and mankind's other endeavors cannot be calculated.

The results of the SCG deliberations and the second LDR workshop at Asilomar, summarized in this publication, represent a further stage in a continuing effort to define the scientific possibilities and the engineering requirements of this next-generation space structure and to meet the challenge of constructing this new eye on the universe.

B. COMPARISON OF THE LDR WITH CURRENT AND FUTURE INFRARED AND SUBMILLIMETER TELESCOPES

A 20-m diameter LDR would provide approximately 20 times better spatial resolution and 400 times more collecting area than the 1-m class airborne and balloon-borne telescopes now in use at wavelengths between 30 and 600 μm . The increased aperture leads to a dramatic jump in scientific capability and makes possible the exciting and important investigations described herein. At some wavelengths between 300 μm and 1 mm it is sometimes possible, though difficult, to observe from mountaintop sites; large ($\geq 10\text{-m}$) ground-based telescopes for this purpose will certainly come into use before the LDR is launched.

Like the airborne and balloon-borne telescopes, these ground-based telescopes will be important scientific and technical precursors of the LDR; however, the atmospheric windows in the 300- μm to 1-mm range are narrow and variable; at best, transmission in these windows rarely exceeds 30 percent. For most purposes, the total freedom from atmospheric effects should make the LDR much more powerful than a comparably sized ground-based telescope, which, in any case, could not operate in the LDR primary range of 30-300 μm where the terrestrial atmosphere is opaque. Besides the large ground-based single dishes now being constructed, there are several millimeter-wave interferometers coming into use. These instruments can produce molecular line and continuum

maps at angular resolutions down to 1 arcsec. These instruments can, however, only be used longward of about 1 mm so that the LDR will be essential to obtain information in the important region of maximum energy output of the star formation regions and galaxy nuclei studied at long wavelengths by the ground-based instruments.

The LDR will also complement other space telescopes planned for infrared observations over the coming decade. Comparison of the LDR with the Cosmic Background Experiment (COBE), the Infrared Astronomical Satellite (IRAS), and the Space Infrared Telescope Facility (SIRTF)--each cryogenically cooled and, therefore, very sensitive--is particularly instructive. COBE is designed explicitly to study the diffuse cosmic background radiation. Its three instruments span the wavelength range from 1 μm to 1 cm. The highest angular resolution achievable from COBE will be 1 deg; its findings will thus be complemented by the 3600 times higher angular resolution provided by the LDR. These fine-scale measurements are crucial for investigating small-scale signals from the galaxy-formation epoch.

In 1983 IRAS surveyed almost the entire infrared sky with a cryogenically cooled 0.6-m telescope. The IRAS catalog contains positional and brightness information for some 250,000 individual point sources and produced total intensity images in four wavelength bands between 12 and 100 μm . However, IRAS had relatively poor spatial resolution ($\sim 1\text{--}2$ arcmin) and no spectroscopic capability longward of 30 μm . The LDR will be able to study IRAS sources in great detail and will provide the first information on their structure and kinematics on the 1-arcsec scale.

SIRTF (size approximately 1 m) will be an observatory-class facility with three focal-plane instruments; its very cold (10 K) optics will make it 100 to 1000 times more sensitive than presently existing infrared instrumentation from 5 to 200 μm . It is primarily a broadband imaging or low spectral resolution telescope. SIRTF will therefore open many new fields for study and exploration in the infrared. The LDR is designed to be especially effective for high spectral resolution in the submillimeter ($\lambda \gtrsim 100$ μm) range, where telescope cooling is not so important, and possibly also at 1-4 μm , shortward of its own emission peak. The spatial resolution of the LDR will be far

greater than that of SIRTf. The LDR will also be much more sensitive for high resolution spectroscopic observations; the temperature of the optics matters little at very high spectral-resolving power.

Finally, in this section on comparisons, it should be mentioned that the Hubble Space Telescope (HST) will have a second-generation, focal-plane infrared instrument. Although the HST will have better angular resolution in the near-infrared than can be obtained by the LDR in its possible light bucket mode, because of its much larger collecting area, the LDR would still be more sensitive for certain experiments such as the detection of the 1-4 μm radiation from primeval galaxies. The LDR thus occupies a unique and broad domain in "discovery space." Its high angular resolution and high sensitivity in the 30-300 μm range make it the logical tool for detailed exploration of matter assembling into galaxies and stars.

C. HISTORY OF THE LDR

The impetus for the development of the LDR began in the late 1970s with two parallel proposals--one for study of a large submillimeter telescope by the Jet Propulsion Laboratory, and the other for study of a large (infrared) telescope by the Ames Research Center. These proposed studies were united into one, intended to lead to the development of a large-aperture (at least 10-m effective diameter) telescope for far-infrared and submillimeter astronomy.

Discussions with university scientists and representatives of aerospace companies working on related problems indicated that such a telescope would be technically feasible in the 1990s; this conclusion was reinforced by technical studies sponsored by NASA. Even at this early stage in its definition, the LDR was among the projects reviewed by the Astronomy Survey Committee of the National Academy of Sciences. This committee, which was charged with defining a program of astronomical exploration and study extending well into the 1990s, recommended the LDR with high priority as a major new NASA Space Program for development in the late 1980s, saying:

"The Astronomy Survey Committee recommends the construction of a Large Deployable Reflector of the 10 m class in space to carry out observations

in the far-infrared and submillimeter regions of the spectrum that are inaccessible from the ground. A number of important scientific problems are uniquely accessible to such a Large Deployable Reflector in space. For distances less than 500 parsecs, the projected beam diameter will be less than 1000 AU."

"Direct measurements of the sizes of nearby clouds collapsing to become stars will thus be possible at far-infrared wavelengths, which can penetrate the surrounding clouds of dust that invariably obscure small-scale features at optical wavelengths. In addition, the wavelength regions accessible to an LDR contain spectral lines of atoms, ions, and molecules that reflect a wide range of astrophysical conditions."

"Studies of these features will yield otherwise unobtainable information about the structure and dynamics of planetary atmospheres; the heating, cooling and chemical composition of the interstellar medium; and, because of the penetrating power of long-wavelength radiation, chemical abundances in the highly luminous, but optically obscured nuclei of active galaxies."

"The sensitivity and high angular resolution of an LDR will also make it possible to study newly forming stars in optically obscured regions of nearby external galaxies, enhancing our understanding of galactic evolution and of the dynamical processes that stimulate star formation. Such an instrument can also probe the structure of the early Universe and the mechanisms of galaxy formation through studies of small-scale spatial fluctuations in the cosmic microwave background radiation."

It is anticipated that the LDR will be a major national project with far-reaching astronomical and technical ramifications. The first Asilomar workshop made a major attempt to define the scientific rationale for the LDR and to compare the astronomical requirements with the technical possibilities. The large number (~100) of scientists and technologists involved in the workshop and the wide range of topics discussed were evidence of the excitement and challenge of this project. A summary of the science goals developed at this workshop is given in Figure 1-3.

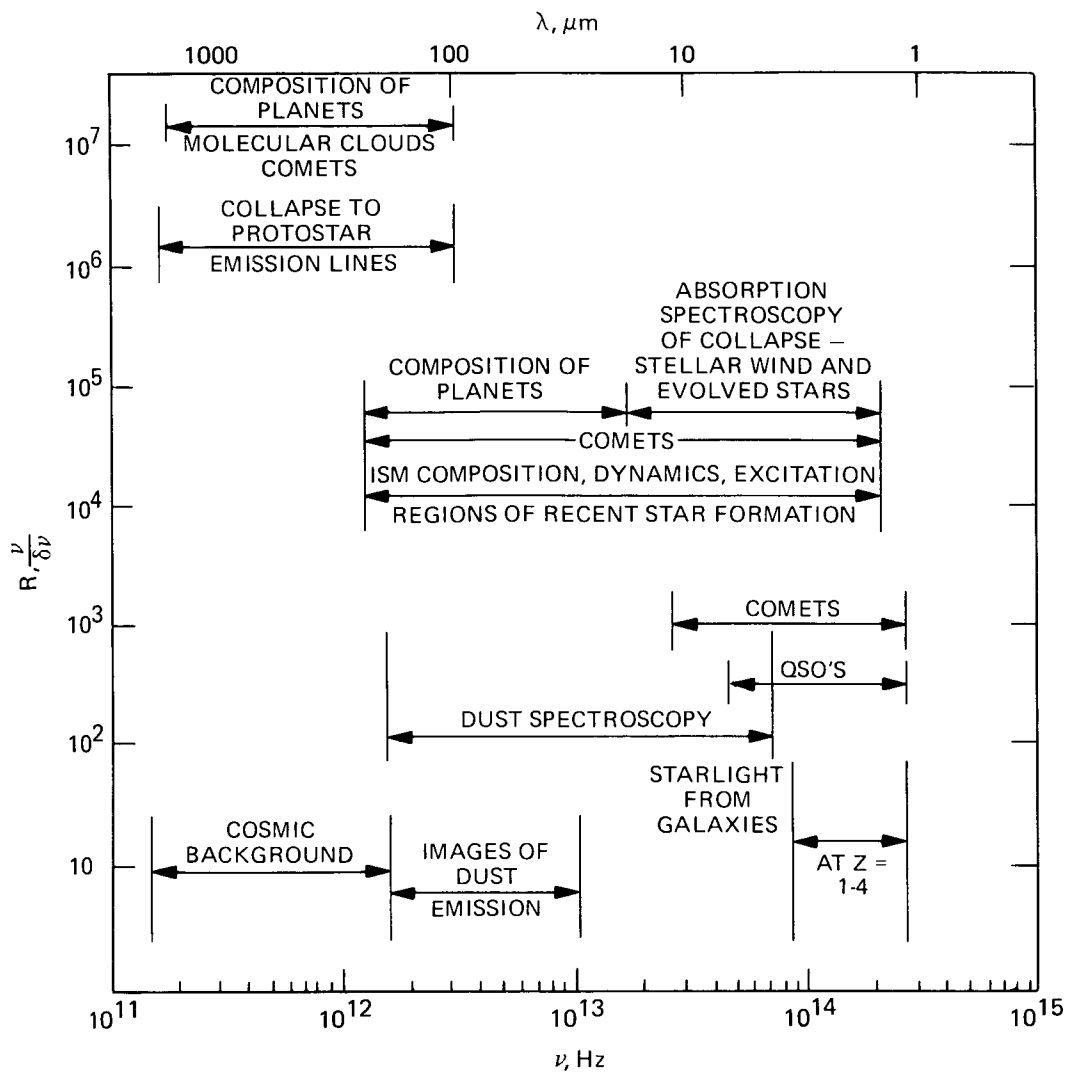


Figure 1-3. Summary of the LDR science objectives

The second Asilomar workshop reviewed the progress on technical issues defined by several independent system and subcomponent design studies which included

- (1) Two major industrial (Kodak and Lockheed) studies of the entire system carried out by NASA through Ames Research Center.
- (2) A major JPL study, again on the entire system.
- (3) A science coordination group study on the focal-plane instruments and cooling.

- (4) Science coordination group studies on the light bucket mode and interferometer mode.
- (5) Science coordination group studies on a multiplicity of further technical aspects.

In addition, the workshop considered changes in the science role of the LDR, particularly as a result of the successful IRAS mission. A brief introduction to some of these issues now follows.

D. RECENT LDR ISSUES

1. NASA Technical Review

The industrial studies were carried out by Lockheed and Itek companies as one team and Kodak and McDonnell Douglas as another team. The results were fairly similar in that both recommended glass mirror primaries of high accuracy, Space Station astronaut-assisted deployment, and more than one Shuttle launch for lifting the LDR total system. The reports will be presented to NASA separately and will not be reviewed extensively here. However, the Science Coordinating Group (SCG) did interact with the contractors throughout the study process and was able to influence the results in some aspects. The general feeling of the SCG was that the contractor systems were somewhat constrained by the choice of glass mirrors and therefore became heavy and bulky, resulting in large costs and Shuttle loads.

The JPL study was carried out in response to the SCG feeling that the industrial studies did not adequately examine the possible gains provided by lightweight, inexpensive structures of lower final surface accuracy. The main features of the JPL study are the use of modern lightweight panels of a composite structure for the primary mirror and a two-stage optical configuration to allow correction for primary errors to be made with a smaller, less expensive subsystem.

2. Space Station Assembly of the LDR

The LDR was originally conceived as a free-flyer, transported to orbit by the Space Shuttle and deployed to its final configuration largely autonomously. It is now likely that the Space Station will be operational by the time the LDR is launched. This opens up the other LDR possibilities of a Space Station attached co-orbiting platform or a Space Station assembled free-flyer.

The Space Station assembled free-flyer at an orbital altitude of ≥ 700 km is presently the baseline design. The lower altitude Space Station attached option has been ruled out because of severe contamination and oxygen erosion problems and the rapid orbit decay times (and consequent requirements for frequent reboosting).

The LDR will be partially assembled and functionally tested on the ground. It will then be disassembled and packed into containers or holding fixtures for installation in the Shuttle bay. The Shuttle will transport the LDR pieces to the Space Station where they will be temporarily stored for later assembly. The individual pieces need not all be brought up to the Space Station on the same Shuttle. The LDR, after assembly and checkout, will be boosted to its ≥ 700 -km orbit by the orbital maneuvering vehicle (OMV).

During the lifetime of the LDR, it will be revisited for on-orbit servicing, instrument changeout, cryogen resupply, general repairs, and orbital reboost.

Details of the aforementioned scenario, along with the requirements that the LDR places on the Space Station for staging assembly checkout, are given by Mattingly (1986).

3. SCG Focal-Plane and Instruments Study

In order (over the lifetime of the LDR observation) to fulfill the scientific goals illustrated in Figure 1-1, eight instruments of the following characteristics were thought by the SCG to represent a minimum package. These

instruments include two detector-array cameras for broadband imaging at high spatial resolution, two direct-detection spectrometers to provide broad spectral coverage and imaging capability at medium spectral resolution, and four heterodyne receivers for spectroscopy and imagery at high spectral resolution. The nature of these systems is discussed at length in the following paragraphs.

There are so many interacting problems, even at the level of the instruments and the focal plane, that it is of primary importance to find a reasonable overall configuration for the instrument package. Probably the major consideration in this task is the nature of the cryogenic system needed by each of the eight instruments. We have assumed for our baseline approach that a central cooling engine will be available which can provide both 20-K and ≤ 4 -K coolants to the individual instruments. This is necessary to preserve the on-orbit instrument changeout capability, which is considered essential for a long-lived (>10 years) national facility. The cryogenic technology is quite critical and, although outside the specific expertise of the SCG, is addressed in general terms in Section IV.

Figure 1-4 indicates the provisional configuration of the focal-plane instrumentation. A summary of the specification of the instrument package follows:

- (1) 8 instruments, each housed in an independent module
- (2) Instrument space: 2 x 1.3 x 0.95 m each
- (3) Total instrument volume: electronics and cooler are 3 x 3 x 4 m
- (4) Total instrument mass: 1600 kg; electronics and cooler mass: 400 kg
- (5) Instrument modules change out, in orbit, independently
- (6) Generic cooler or LHe pump feeds coolant to modules independently
- (7) Rotating mirror feeds light to the eight stations, with several subpositions

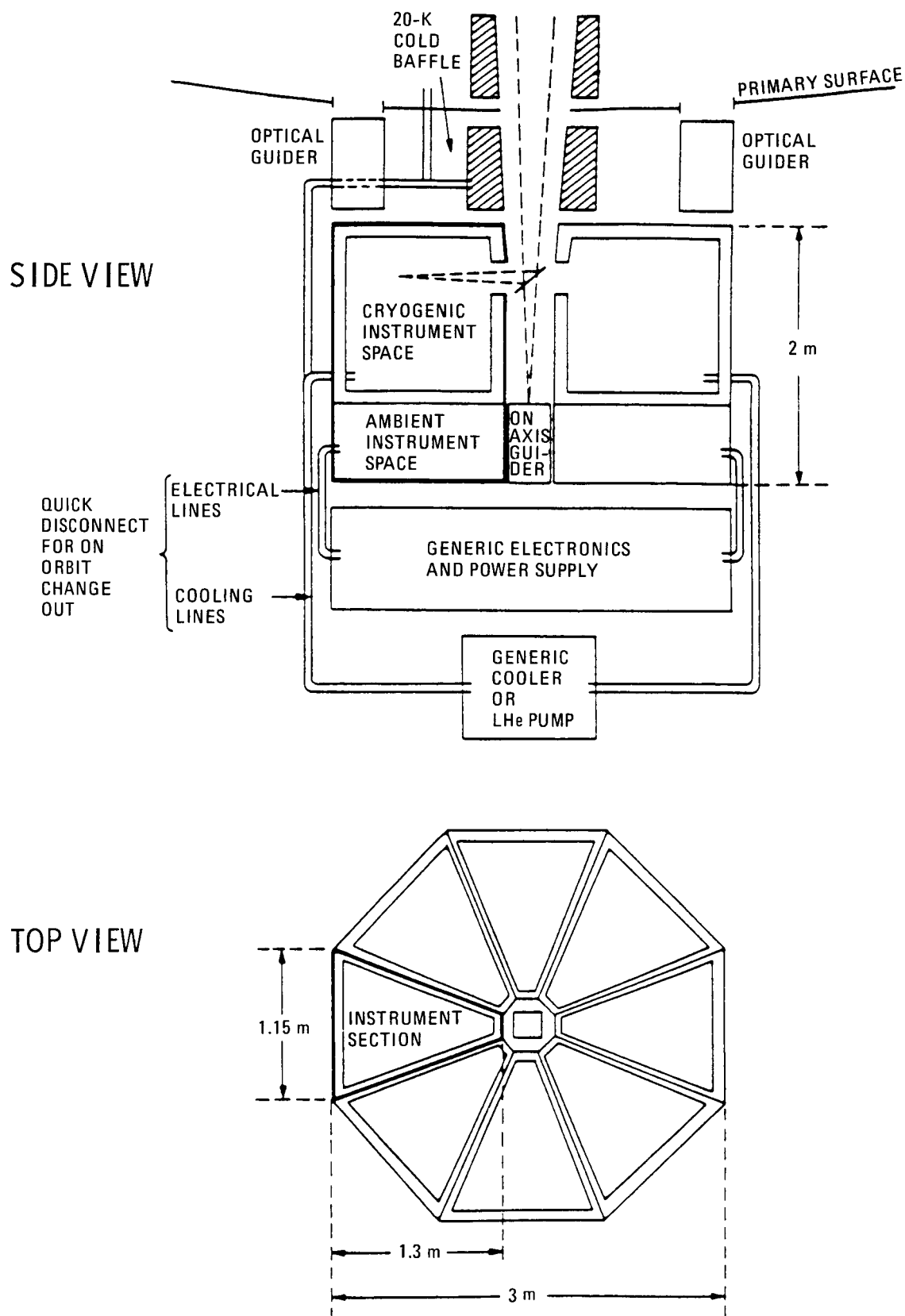


Figure 1-4. Provisional configuration of focal-plane instrumentation

- (8) Optical guiders look through the primary mirror, past the edge of secondary mirror
- (9) Size of 4-K instrument space to be minimized for each instrument
- (10) Division of space between cryogenic and ambient is arbitrary

Although Figure 1-4 shows eight instruments at the LDR focal plane, the Asilomar II instrument panel and the SCG felt that realistically there may only be four instruments on the LDR at any one time. Instrument changeout on orbit is essential and, over a period of several years, all eight instruments would be circulated through the focal plane. This would allow upgraded instruments as well as new instruments to be installed.

4. The Light Bucket Mode

The original specification for the LDR called for a ~ 1 -arcsec performance in the $\lesssim 4\text{-}\mu\text{m}$ wavelength range. This light bucket requirement was found to drive the panel requirements in a way that might have a major impact on cost and practicality. The light bucket performance is now considered as a goal as long as it does not seriously (>20 percent) impact the cost.

5. The Interferometer Mode

During the course of the SCG studies it became clear that certain subsets of the science goals could only be met by an interferometer style instrument rather than by a single reflector. These were the ultrahigh angular resolution studies of forming stars. For many cases it was considered preferable to have 0.1- to 0.01-arcsec resolution as compared with the ~ 0.5 arcsec, which is the best that the LDR can provide at $50\text{ }\mu\text{m}$. The SCG considered the question with the following alternatives in mind:

- (1) Converting the LDR totally to an interferometer system of several electronically or optically connected small elements.

- (2) Adding a set of small electronically connected mirrors as a "satellite" with a single-dish LDR as a central element. (This mode could be called a hybrid.)

The main arguments against the pure interferometer mode (Item 1) seemed to be that it traded one set of technical difficulties for another and that the new set might be even harder to overcome. Also, while high resolution experiments on bright sources could be done better by an interferometer, a much larger range of experiments requires sensitivity rather than resolution and could not be done with the multiple "small dish" LDR. Finally, the cost and difficulty of providing high sensitivity, wide spectral coverage instruments on each of the multiple optical elements would be overwhelming, and the problems of optical beam combining to a central detection and processing facility seemed outside the knowledge of the science community.

Option (2) would suit all science goals. The main considerations then become questions of costs and technological approach. If the central LDR were to be reduced in aperture, in order for the hybrid to match the cost of the 20-m LDR project, considerations of the science reduction as a function of aperture would have to be more precise. The SCG was firm in its belief that the nominal effective collective area of 20 m shall not be compromised. Moreover, there was an unquestioned but strongly held belief that initial high-sensitivity, high-angular resolution studies at submillimeter wavelengths should be carried out first with a single dish.

The additional (and unknown) technical complexity of designing a space-based interferometer seemed unjustified given our ignorance regarding the spatial scales and corresponding surface brightness (line + continuum) characterizing the submillimeter sky.

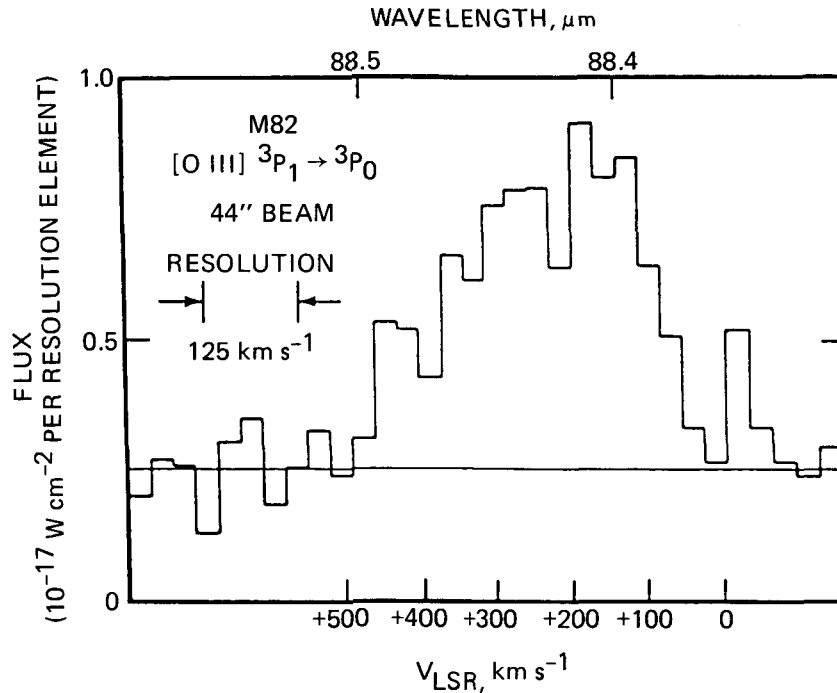


Figure 2-8. OIII line emission from M82

both grating and Fabry-Perot instruments. With the LDR, heterodyne instruments will be available to fully resolve the velocity structure. Figure 2-9 shows a set of spectra taken at various points in M82 indicating that, on the whole, the CII distribution is similar to that of CO (Crawford et al., 1984).

The CII line often contains several percent of the total luminosity of a spiral galaxy. This line concentrates so much energy in such a narrow bandwidth that the LDR will detect it even from galaxies at $Z = 1 - 2$. Study of this line at various epochs may prove to be one of the principal applications of the LDR. Later we discuss how the size of the LDR is specified by the resolution needed at this wavelength. If this line can be studied at high redshifts with a velocity resolution of a few tens of kilometers per second, we will be able to use the Fisher-Tully relation to derive distances, and so measure both H_0 and q_0 with unprecedented accuracy. In addition, we may be able to study directly the chemical evolution of galaxies back to the epoch of galaxy formation.

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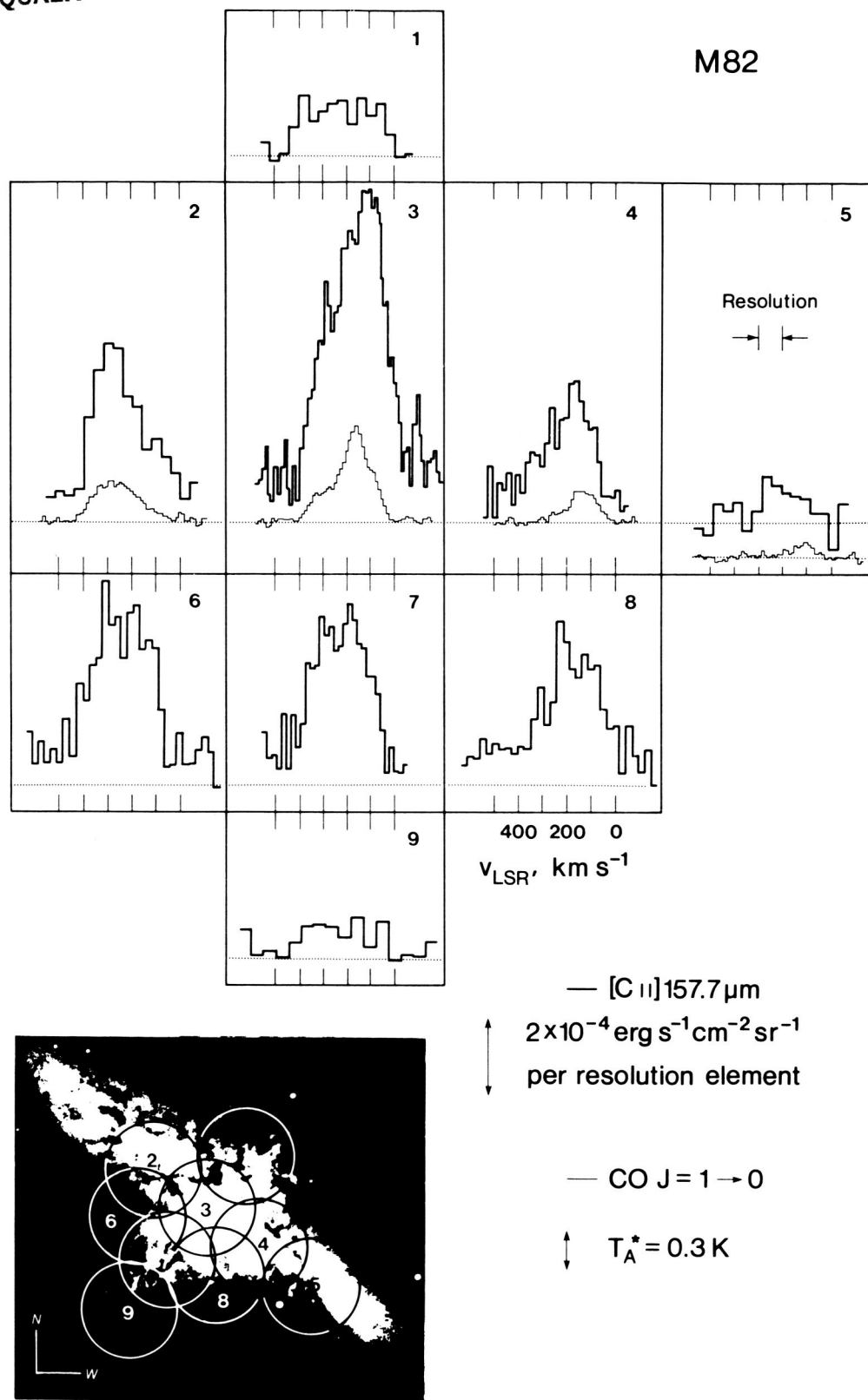


Figure 2-9. CII and CO line emission from M82

AGNs are very luminous in the far-infrared. High-spatial-resolution observations can investigate whether the far-infrared emission comes from close to the central "engine" as the near-infrared and optical emission, or whether the influence of the active nucleus has triggered violent star formation in the larger disk of the galaxy. Similar questions can be asked about the new classes of quasars discovered by IRAS. In 3C48 it appears that an active star-forming galaxy surrounds the quasar, but in the case of the infrared quasar 13348+2439 it appears that the interstellar medium in a dusty spiral galaxy absorbs the ultraviolet light of the quasar, converting it directly into infrared emission. High-spatial-resolution observations in the far-infrared can provide images and probe the physical conditions within the gas-rich galaxies associated with AGNs and some quasars.

SECTION III

SYSTEM REQUIREMENTS AND TRADES

The SCG has considered several aspects of the LDR system with respect to the impact of the science goals. Although these system aspects are obviously multiply-connected in terms of their effects on the science, we report separately upon them.

A. APERTURE VS SCIENCE

There are persuasive scientific arguments for making a 20-m, rather than a 10-m diameter, LDR. The LDR science goals drive the need to achieve both the greatest spatial resolution and sensitivity. Generally speaking, the feasibility of difficult observations goes like D^4 . Some crucial observations that can be done with a 20-m LDR become impossible with a 10-m aperture. Two scientific questions, the study of galaxies at high redshifts ($z > 1$) and the search for extra-solar planets, illustrate the importance of a large-diameter LDR.

The number of molecular line sources at the distance of the Virgo cluster or beyond that could be studied by the LDR decreases dramatically as the diameter is reduced below 20 m. For example, to obtain the submillimeter molecular spectrum of a distant galaxy, we would expect the nucleus of a typical spiral galaxy (central 500-pc region) to have an observed brightness temperature of ≥ 100 mK in the stronger molecular lines. When the telescope beam takes in the whole nucleus, such galaxies would be detectable with a 20-m diameter telescope to a distance of about 25 Mpc. Since anticipated receiver noise temperatures are several hundred kelvins, a significant number of Virgo cluster galaxies will be detectable. However, reduction of the diameter to 10 m would reduce even the strong lines to ~ 25 mK and make the Virgo cluster project (much less more distant objects) essentially unfeasible.

The LDR also offers the possibility of studying the early evolution of spiral galaxies at redshifts greater than $z = 1$. Far-infrared measurements could be used to study the evolution of the star formation process as a

function of look-back time; high-spectral-resolution measurements in the CII line at a rest wavelength of $158\text{ }\mu\text{m}$ could determine the masses of distant spiral galaxies and measure the geometrical properties of the universe itself. However, to accomplish these goals, the spatial resolution of a 20-m aperture is required. In a cluster of galaxies at a redshift of $z = 1$, the typical galaxy diameter is about 1 arcsec, and the separation between galaxies is 20 arcsec. To measure a redshifted galaxy at its rest-frame emission peak wavelength of $100\text{ }\mu\text{m}$, an observation must be made at $200\text{ }\mu\text{m}$ where the beam size (FWHM) of the 20-m LDR is 4 arcsec. To make an unconfused measurement of an individual object requires a minimum separation between objects of about five beam widths. Thus, an LDR smaller than 20 m will be confusion limited by cluster galaxies and be unable to make the necessary observations.

Evolutionary effects in the rate of star formation become clear for galaxies at redshifts of $z = 1$ or greater, corresponding to eras 10 billion years ago. For closer, older galaxies, these effects are much less pronounced. IRAS showed that a typical spiral galaxy in the Hercules cluster ($z = 0.04$) emits about 50 mJy at $60\text{ }\mu\text{m}$. At a redshift of 1, such an object would produce about 160 μJy at $120\text{ }\mu\text{m}$, which would be detectable in about 1 hour with a signal-to-noise ratio of 10 with the 20-m LDR. Observing such galaxies with a smaller LDR would quickly become an impractical task, requiring days of integration time.

Perhaps the strongest spectral line from the warm interstellar gas found in spiral galaxies is the $158\text{-}\mu\text{m}$ transition of singly ionized carbon, CII. Our own galaxy emits 0.1 percent of its entire luminosity in this transition (Stacey et al., 1985). The $158\text{-}\mu\text{m}$ CII line can be used to characterize the thermodynamics of the interstellar medium and to give the total mass of the galaxy. Using the LDR, the CII line can be used to determine the properties of spiral galaxies out to the edge of the known universe. At $z = 1$ a galaxy like our own could be measured with a signal-to-noise ratio of 5 in 1 hour of integration time with 30 km/s spectral resolution using a 20-m aperture. Because of confusion with other cluster members, the small beam size of the 20-m LDR (3 arcsec at $300\text{ }\mu\text{m}$) is crucial for this program. Once calibrated by the study of relatively nearby galaxies, the CII line can be used to

measure the metric of the universe itself using the Fisher-Tully relationship, which relates to the width of the CII line and the total luminosity of the galaxy.

To date, there is no clear observational evidence for the existence of evolved planets orbiting stars other than our sun. While IRAS has detected shells of dust and debris orbiting nearby stars such as Vega and Beta Pictoris, it lacked the high sensitivity and spatial resolution to detect the thermal emission from a single Jupiter-like planet. The detection of planets around a statistically significant number of nearby stars would be possible with a 20-m LDR. It would quantify the probability of planet formation, and thus greatly constrain the theories of planet formation. In addition, for the first time, the number of planets in the galaxy could be accurately estimated, a number of great significance in the search for extraterrestrial life.

The direct detection of the thermal emission at two wavelengths from extra-solar planets yields the temperature of the planet, and thus its size can be estimated. Long-term observations yield the orbital period of the planet, which will determine the distance between planet and star. The temperature and distance can be combined to determine the effective emissivity of the planet and infer the existence of atmospheric effects or internal heat sources.

The direct detection of Jupiter-like planets drives the technology requirements of the LDR. The distance to which a 100-K Jupiter-sized planet can be detected in 3-hours' integration time in a broadband observation with a 20-m LDR at 30 μm (the peak of the thermal emission) is approximately 4 pc (and is directly proportional to the telescope diameter). There are 26 stars within 4 pc. Thus, a 20-m LDR is required for a statistically significant search. Assuming diffraction-limited angular resolution at 30 μm , and assuming that the near sidelobes (~ 3 -10 arcsec) can be kept down 40 dB (the planet is typically 10^3 - 10^4 times fainter than the star at 30 μm), the LDR will spatially separate Jupiter-sized planets that lie ≥ 10 AU from these nearby stars. Degradation of angular resolution (either by reducing the diameter of the telescope, by relaxing the diffraction limit at 30 μm , or by increasing the near

sidelobes) would increase this minimum separation, so that a planetary system like our own solar system would then become undetectable.

B. THE INTERFEROMETER OPTION

The LDR, from its inception, has been thought of in terms of a single circular-filled aperture. Yet, alternatives must be considered. The clearest alternative is one that has been chosen by many ground-based radio and millimeter-wave facilities: an interferometer array consisting of separate telescopes and generating interference data by means of delay lines and correlators operating at radio frequencies. Michelson interferometry, though briefly considered by the SCG, was considered impractical.

The issues are several, including science capability, technology limitations, and mission design. Interferometry offers improved spatial resolution, though not for all types of observation. Interferometers are complex, so that a meaningful comparison with a filled-aperture instrument must identify what can be accomplished at fixed cost. If radio techniques are used, then each interferometer element must be equipped with heterodyne instruments, and there must be a central facility to provide for appropriate delays and for multichannel spectral correlators. Finally, the added scientific capability cannot be considered in isolation. The LDR has a considerable set of scientific goals and corresponding science instruments. Telescope scheduling among competing instruments is an issue new to orbiting observatories, but one which must be met head-on for a complex observatory such as the LDR.

The advantage in a multielement array, as opposed to a two-element interferometer, is in quickly reconstructing an image and in the use of "phase closure" for elements whether they be free-flying, tethered, or rigidly connected. The signals would have to be correlated in space, as opposed to the concept of mixing in space at each of the elements and then transmitting the infrared to Earth to be correlated. The latter is impractical in terms of the required bandwidth, far exceeding the projected capability, for example, of the Tracking and Data Relay Satellite (TDRS).

A major concern for the LDR heterodyne interferometer would be instrument costs. The estimated cost of the total instrument package for the 20-m LDR is about \$100 million for each of eight instruments in the same range per instrument for those on SIRTf and ST. The cost is higher with the heterodyne interferometer since duplicate instruments for each element are required.

Another technical issue for a space-based array is to know, from moment to moment, the array geometry to a precision of a fraction of an operating wavelength. Earth-based interferometers assume the Earth to be perfectly rigid over the course of an observation, and they characterize the interferometer through a series of calibrating observations. In space, large "rigid" or tethered structures seem difficult to achieve and might introduce large-amplitude vibrations. One approach is to accept a free-flying array in which the relative separations of the elements vary smoothly according to their slightly differing orbits. An orbit analysis indicates that acceptable amounts of propellant could serve to cover the ultraviolet plane. Inter-telescope spacings might be measured by laser ranging, but the needed angular precision is several milliarcseconds, a daunting technical challenge. Use of an ultra-accurate star catalog such as that projected from Hipparcos might help for the case of free-flying elements, but must be considered speculative at this time.

Three specific configurations of comparable estimated cost were considered by the SCG in its study of space-based interferometry: (1) the fully instrumented 20-m filled aperture LDR, (2) an array of nine 4-m antennas (the "array"), assembled on a structure of 200-m extent and containing only single-element heterodyne mixers, and (3) a hybrid mission (the "hybrid"), consisting of a central 12-m element with full instrumentation and four 4-m elements with heterodyne mixers only.

1. The Array

An excellent discussion of some science goals that might apply to the array is presented in a report of the Submillimeter Telescope Committee of the Smithsonian Astrophysical Observatory. Although the wavelength coverage

of an LDR interferometer would differ from that in the ground-based instrument under study, many of the science goals would be similar. Largely, this similarity, which does not apply to the bulk of LDR science, results from the assumption of fixed-cost alternatives and, thus, from the limited instrumentation available to the array. Most of the LDR instruments would not lend themselves to interferometry. Bulk detectors and grating or Fabry-Perot spectrometers are not usable; neither are heterodyne arrays, chiefly because of data-handling problems. Even for single heterodyne mixers, the spectral coverage would have to be limited to fewer spectral channels. A 100-channel spectrometer with 36 interferometer baselines presents a formidable challenge for space-based data management. The conclusion of the SCG was that the science goals addressable by the array made it unacceptable as an alternative to the LDR, especially in the context of a millimeter/submillimeter interferometer construction boom. Most of the LDR-unique science objectives are not met by the array. The small antennas and the focus on longer LDR wavelengths do not sufficiently distinguish the science goals of a space-based array from those of ground-based interferometers. This situation might change after successful exploratory observations of a single-element LDR.

2. The Hybrid

The third, or hybrid, configuration for an LDR interferometer represented a compromise solution which would provide limited interferometer capability with a minimum impact on the many LDR-unique science objectives. It consists of a central, fully instrumented 12-m reflector and four 4-m elements with only single-element heterodyne mixers. The hybrid adds interferometer capability while preserving much LDR-unique science. However, the SCG felt that the penalties of operating such an unbalanced heterodyne array outweighed its advantages.

The first and most serious shortcoming of the hybrid is that it makes an inferior interferometer as compared to an array of identical antennas: (1) only four of the ten available interferometer baselines would include the central antenna, which contains over 3/4 of the total collecting area, (2) the field of view of the interferometer is restricted by its largest element,

whereas (3) the sensitivity on any interferometer baseline is equal to the geometric mean of the antenna diameters, more like that of the smaller element. Thus, for the hybrid, mapping would take 2.5 times longer than for a five-element array of equivalent total antenna collecting area, because of the restricted baseline coverage. The field of view, restricted by the control element, is only 11 percent of that in the array, restricting the number of pixels recoverable from a synthesized map. Finally, the large gain of the central element is not achievable on most baselines in the hybrid, restricting the detectable flux on these baselines.

The hybrid also encounters two other difficulties in comparison to the LDR. These have to do with mission design and with technology development problems. The central element of the hybrid is assumed to contain the full instrument complement. The interferometer elements contain only single heterodyne mixers. Even with the LDR, it was felt that eight instruments were possibly too much to mount at one time. In large part, this decision came about because of telescope scheduling problems; the realization that the observing needs of each instrument could only be efficiently met 12 percent of the time. The interferometer would make this problem worse. Full aperture synthesis is necessary for the complex source structures in the interstellar medium. This has been demonstrated by experiences with the OVRO and the Hat Creek interferometers. Such observations are slow. It would not make sense to build a hybrid, only to use it 20 percent of the time or less. Yet there are other pressing demands for the central LDR. In practice, it was felt that maximum scientific gain would result by using the four elements of the hybrid by themselves, continuously, and with a large field of view. But in that case, there is no reason to consider the four elements to be part of the LDR project. Thus, the hybrid, when faced with a real mission design, seems to divide itself into two separate projects: the LDR, and a later, separate array.

The final consideration which must be given to the hybrid is that of needed technology. The hybrid contains a large central reflector in addition to the full LDR instrument complement. That recreates all of the instrument and cryogenic needs, in addition to most of the controls and optics problems. In addition, the hybrid has all of the special needs of the array: multi-channel, multibaseline correlators on a limited power and weight budget and

the problems of maintaining and measuring the interferometer baselines in the absence of a solid foundation (e.g., the Earth).

3. Interferometer Science Discussion

The science goals for the LDR were reviewed in light of the capabilities of the LDR, the array, and the hybrid. It is useful to keep the sensitivity calculations in mind. For the array, we assume $N = 9$ antennas of $d = 4$ -m diameter each, with a maximum separation of $D = 200$ m. To be conservative we take a heterodyne detector bandwidth of $\Delta\nu = 2$ GHz at a wavelength of $100 \mu\text{m} = 3000$ GHz. A system temperature of $T_{\text{sys}} = 1000$ K and an integration time of $\Delta t = 4$ hours are assumed. The $1\text{-}\sigma$ rms continuum detection limit is then about $T_{\text{source}} \sim 100$ mK. The result scales as

$$T_{\text{source}} \sim \frac{D^2 T_{\text{sys}} d^{-2}}{\sqrt{\Delta\nu \Delta t N(N-1)}}$$

The angular resolution for the example is $\theta \sim 0.1$ arcsec and is proportional to D^{-1} . In other words, a dust map of a distant galaxy could be obtained at 0.1-arcsec resolution in 4 hours, to a 100-mK level. Unfortunately, this is comparable to or above the expected level of source temperature for most distant galaxies (see below). The hybrid sensitivity is more complicated, since not all baselines have the same gain. However, the numbers are similar. A preliminary cut was taken at the science, which is summarized below. We assumed the hybrid configuration in order not to sacrifice the bulk of LDR science, achieved with the full strawman instrument complement. The science was approached with the question "How much do we give up going from 20 to 12 m, and how much do we gain with the interferometer?"

a. Cosmology. Anisotropies in the cosmic microwave background at $\lambda \sim 1$ mm (including the Sunyaev-Zeldovich effect) have interesting scales at 10 arcsec to 1 arcmin. Thus, a 12-m telescope is nearly as good as a 20-m telescope. Counting highly redshifted dusty galaxies at $\lambda \sim 300 \mu\text{m}$ might require $\lesssim 3$ -arcsec resolution; here a 20-m dish is marginally better, but this experiment is speculative.

b. High Z Galaxies. The optical depth of dust through the disk of a spiral galaxy is $\tau_d \sim 10^{-2}$ at $\lambda \sim 100 \mu\text{m}$. Since the dust temperature is $T_d \approx 10 \text{ K}$, the expected brightness temperature is of order $T_b = T \cdot \tau \sim 0.1 \text{ K}$, not including a Z correction, which can be compared to the above equivalent noise temperature. The tentative conclusion is that dust emission would be too weak to map at high spatial resolution. The optical depth of the CII (158- μm) transition through a spiral galaxy is also of order $\tau \sim 10^{-2}$. The excitation temperature of CII may be as high as 100 K, but the decrease in $\Delta\nu$ for this line makes the noise temperature ~ 30 times higher than in the case for dust emission so this line would also be hard, or impossible, to map at 0.1 arcsec. Interferometry seems to offer little here.

c. Nuclei of Galaxies. QSOs have jets whose size scales range from subparsec to many kiloparsecs. The central engines may have size scales of order astronomical units. Thus, the more resolution the better (1 pc = 0.01 arcsec at 20 Mpc, a "typical" distance to a nearby active galactic nucleus). In our own galactic center, we see very interesting ionized clouds and molecular rings with size scales of order 0.1-1 pc. To look at nearby galaxies to the same spatial scale requires 0.01-arcsec angular resolution. Such resolution may help separate thermal and nonthermal components of the continuum. These observations do not require high sensitivity, but high spatial resolution. Line emission from the fine structure transition of, for example, OIII, CIII, and OI may also be detectable (these will allow velocity measurements, hence the dynamics of the region).

d. Galaxies. A 20-m LDR would do a great deal in looking at global star formation, galactic structure, etc. The questions are "How do giant molecular clouds form?" and "What is the thermal history of interstellar gas?" It may be that a 1-arcsec beam observing nearby galaxies will be enough to answer these questions. No strong arguments for interferometers were unearthed here.

e. The Center of Our Galaxy. The spatial resolution of a 20-m dish is sufficient and its large gain is desirable. The ionized and neutral clouds have size scales ≥ 1 arcsec.

f. The Structure of Our Galaxy. Most structure questions are answered at resolutions ≥ 1 arcsec; thus, a 20-m dish is needed.

g. Chemistry and Interstellar Processes in Our Galaxy. Some projects would be helped by better resolution: (1) searching for molecules with no electric dipole moment like O_2 and CH_4 but which have induced-dipole transitions in the submillimeter and infrared, (2) probing the density peaks in molecular cores for exotic molecules (here the "spatial filtering" helps to untangle the "forest" of lines which will probably be observed), (3) observing at high spectral resolution (0.1 km s^{-1}) and spatial resolution the effects of ambipolar diffusion (different molecules have different slip velocities) and thereby estimating magnetic field strengths, and (4) measuring polarization from the molecular lines.

h. Star Formation. Protostars are thought to have disks around them which form planets. The size of these disks are ~ 10 AU. At the distance to the nearest star-forming region (in Taurus), this corresponds to 0.1 arcsec. To resolve these disks in order to look for condensations which may form planets, to study the accretion shock on the disk (possibly observable through the accreting cloud at $\lambda > 100 \mu\text{m}$), and to really "see" the basic processes of star and planet formation require an angular resolution of 0.01 arcsec (preferably) to 0.1 arcsec. The continuum from the disk is observable with the interferometer at 0.01-arcsec resolution. A calculation needs to be done on the observability of the accretion shock lines. Ideally, we would like to see line emission in order to sort out the dynamics at ≤ 100 AU. These observations represent some of the strongest arguments for interferometric capabilities on the LDR.

i. Outflows. Outflows were discussed in terms of protostellar outflows, as well as outflows from old stars such as red giants, novae, WR stars, Miras, etc. In the former, it appears that the flows are collimated by ≤ 10 -AU disks, and that the flows are clumpy at scales < 1 arcsec and probably < 0.1 arcsec in the nearby regions (Taurus, Orion). H_2O masers are clumps with size scale 10^{14} - 10^{15} cm, which is < 0.1 arcsec at Orion. Around old stars, the great problem is that of dust formation--How does dust form? Does it form in clumps? Here again, 0.1-arcsec resolution is needed and 0.01 arcsec is

preferable. A first guess is that all these observations are not sensitivity limited, rather they require angular resolution.

C. THE LIGHT BUCKET

The science report of the 1982 Asilomar workshop on the LDR suggested the use of a 20-m diameter LDR as a light bucket at wavelengths $\lambda < \lambda_d$, where $\lambda_d = 30$ to $50 \mu\text{m}$ is the shortest wavelength to which the telescope is diffraction limited. In particular, it stressed the application of the LDR to the problem of observing distant high redshift galaxies to the fundamental limit that the galaxy images overlap (the "confusion limit"). In this application, a passively cooled mirror temperature $T_{\text{LDR}} < 200 \text{ K}$ was important. In addition, the report compared the LDR with other telescopes in the $1\text{-}\mu\text{m} < \lambda < \lambda_d$ wavelength range.

Several points have emerged in the last few years concerning light bucket operation of the LDR, which represent new findings or different conclusions from the earlier Asilomar report. Because of the likely development of much better detector sensitivity than was assumed in the Asilomar report, SIRTf gains in advantage over the LDR for low spectral resolution work at $\lambda > 4 \mu\text{m}$ (e.g., distant galaxies). At the same time, the large ground-based telescopes will be able to reach the confusion limit of distant galaxies for $1 \mu\text{m} < \lambda < 2.5 \mu\text{m}$, although with considerably larger integration times than the LDR. Consequently, the science rationale de-emphasizes the use of the LDR as a photometer between $1\text{-}4 \mu\text{m}$, but emphasizes more the use for high resolution spectroscopic mapping in the $1\text{-}30 \mu\text{m}$ range. Good light bucket capabilities, if not diffraction limited performance, are very desirable at $28 \mu\text{m}$ in order to probe the distribution of H_2 . Subjectively, the $\lambda < 25\text{-}\mu\text{m}$ light bucket science, although interesting, does not seem to warrant a major increase in the cost of the LDR.

The cost of achieving a ~ 1 arcsec light bucket capability for $1 \mu\text{m} < \lambda < 25 \mu\text{m}$ is uncertain at present. Each 2-m segment must individually achieve a spot size of ~ 1 arcsec; alignment of the spots from each segment to within ~ 1 arcsec will automatically be achieved if the telescope is diffraction limited at $30\text{-}50 \mu\text{m}$. The increased cost of producing optical

quality 2-m segments vs 30- μ m diffraction-limited segments has yet to be determined. The importance of passively cooling the telescope is de-emphasized. The pixel size ($\geq 100 \mu$ m) for a 1-2 arcsec light bucket may be so large as to degrade detector performance.

In light of the above scientific and technical considerations, we suggest a "goal" that the LDR have the "light bucket capability" to focus 50 percent of the $1 \mu\text{m} < \lambda < 30 \mu\text{m}$ radiation from a distant point source into an image diameter $\theta_{\text{LB}} \leq 1$ arcsec if the increased cost on the LDR system to achieve this capability does not exceed 20-30 percent of the total cost and if no major delays in the LDR program are affected.

D. INSTRUMENTS AND CRYOGENS

The SCG subgroup on instruments provided a strawman focal-plane configuration (described in Section I of this publication and in JPL Internal Document D-2214). Arguments and calculations were provided to indicate what instruments would be required to satisfy the science goals of the LDR and how the instrument program should be implemented. Both the current status and development needs were described in JPL Internal Document D-2214. A summary of the document is provided here.

1. Instruments

The astronomical projects outlined in the Asilomar reports require that the LDR be equipped with a combination of instruments capable of observing line profiles ranging in intrinsic velocity width from a fraction of a kilometer per second to several hundred kilometers per second, as well as broadband cameras capable of using the full spatial resolution of the 20-m telescope. The means by which this range of resolutions is achieved must be consistent with the primary goal: sensitivity which approaches as closely as possible the fundamental limits, over the entire wavelength range of the LDR. By way of introduction to the "strawman" package of instruments, we first discuss in general terms the available types of detectors and detection schemes and provide a comparison of their performances. Because the LDR is a warm telescope ($T = T_{\text{ambient}} \sim 200 \text{ K}$), the noise sources which limit

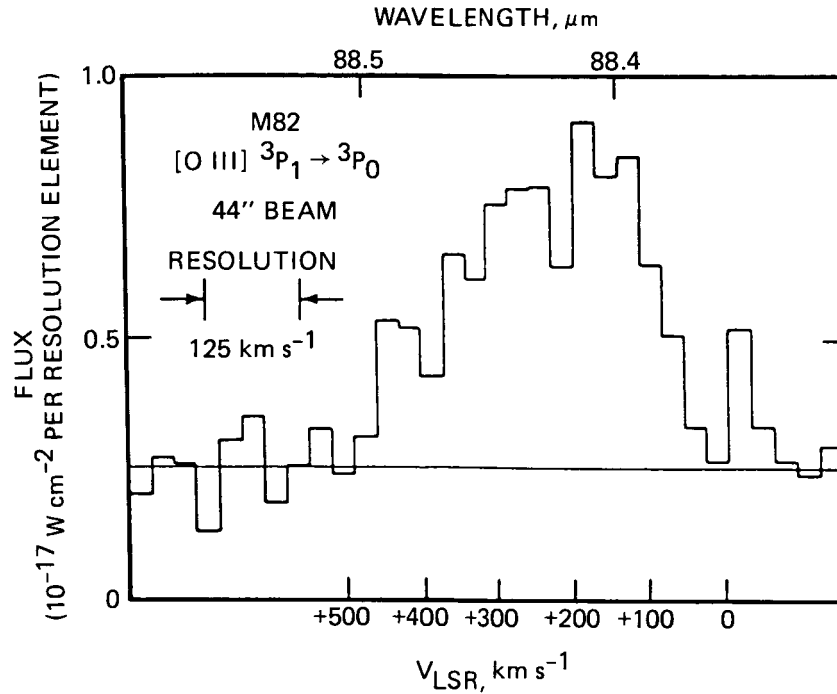
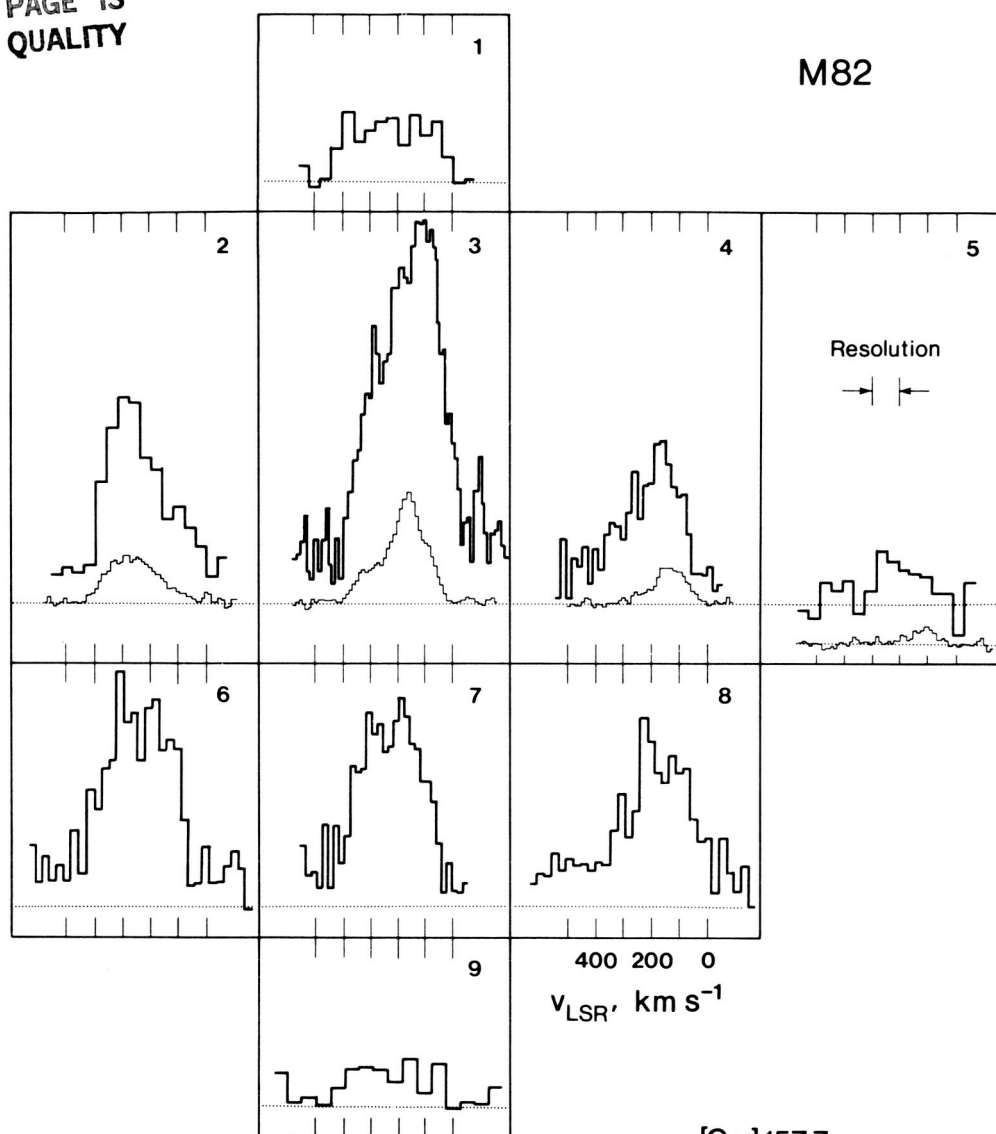


Figure 2-8. OIII line emission from M82

both grating and Fabry-Perot instruments. With the LDR, heterodyne instruments will be available to fully resolve the velocity structure. Figure 2-9 shows a set of spectra taken at various points in M82 indicating that, on the whole, the CII distribution is similar to that of CO (Crawford et al., 1984).

The CII line often contains several percent of the total luminosity of a spiral galaxy. This line concentrates so much energy in such a narrow bandwidth that the LDR will detect it even from galaxies at $z = 1 - 2$. Study of this line at various epochs may prove to be one of the principal applications of the LDR. Later we discuss how the size of the LDR is specified by the resolution needed at this wavelength. If this line can be studied at high redshifts with a velocity resolution of a few tens of kilometers per second, we will be able to use the Fisher-Tully relation to derive distances, and so measure both H_0 and q_0 with unprecedented accuracy. In addition, we may be able to study directly the chemical evolution of galaxies back to the epoch of galaxy formation.

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OF POOR QUALITY



— [C II] 157.7 μm
 $2 \times 10^{-4} \text{ erg s}^{-1} \text{ cm}^{-2} \text{ sr}^{-1}$
 per resolution element

— CO $J = 1 \rightarrow 0$

$T_A^* = 0.3 \text{ K}$

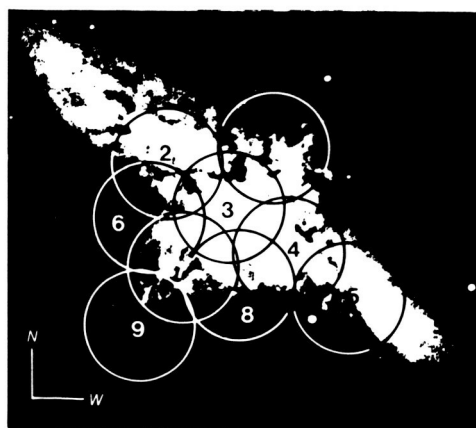


Figure 2-9. CII and CO line emission from M82

AGNs are very luminous in the far-infrared. High-spatial-resolution observations can investigate whether the far-infrared emission comes from close to the central "engine" as the near-infrared and optical emission, or whether the influence of the active nucleus has triggered violent star formation in the larger disk of the galaxy. Similar questions can be asked about the new classes of quasars discovered by IRAS. In 3C48 it appears that an active star-forming galaxy surrounds the quasar, but in the case of the infrared quasar 13348+2439 it appears that the interstellar medium in a dusty spiral galaxy absorbs the ultraviolet light of the quasar, converting it directly into infrared emission. High-spatial-resolution observations in the far-infrared can provide images and probe the physical conditions within the gas-rich galaxies associated with AGNs and some quasars.

SECTION III

SYSTEM REQUIREMENTS AND TRADES

The SCG has considered several aspects of the LDR system with respect to the impact of the science goals. Although these system aspects are obviously multiply-connected in terms of their effects on the science, we report separately upon them.

A. APERTURE VS SCIENCE

There are persuasive scientific arguments for making a 20-m, rather than a 10-m diameter, LDR. The LDR science goals drive the need to achieve both the greatest spatial resolution and sensitivity. Generally speaking, the feasibility of difficult observations goes like D^4 . Some crucial observations that can be done with a 20-m LDR become impossible with a 10-m aperture. Two scientific questions, the study of galaxies at high redshifts ($z > 1$) and the search for extra-solar planets, illustrate the importance of a large-diameter LDR.

The number of molecular line sources at the distance of the Virgo cluster or beyond that could be studied by the LDR decreases dramatically as the diameter is reduced below 20 m. For example, to obtain the submillimeter molecular spectrum of a distant galaxy, we would expect the nucleus of a typical spiral galaxy (central 500-pc region) to have an observed brightness temperature of ≥ 100 mK in the stronger molecular lines. When the telescope beam takes in the whole nucleus, such galaxies would be detectable with a 20-m diameter telescope to a distance of about 25 Mpc. Since anticipated receiver noise temperatures are several hundred kelvins, a significant number of Virgo cluster galaxies will be detectable. However, reduction of the diameter to 10 m would reduce even the strong lines to ~ 25 mK and make the Virgo cluster project (much less more distant objects) essentially unfeasible.

The LDR also offers the possibility of studying the early evolution of spiral galaxies at redshifts greater than $z = 1$. Far-infrared measurements could be used to study the evolution of the star formation process as a

function of look-back time; high-spectral-resolution measurements in the CII line at a rest wavelength of $158\text{ }\mu\text{m}$ could determine the masses of distant spiral galaxies and measure the geometrical properties of the universe itself. However, to accomplish these goals, the spatial resolution of a 20-m aperture is required. In a cluster of galaxies at a redshift of $z = 1$, the typical galaxy diameter is about 1 arcsec, and the separation between galaxies is 20 arcsec. To measure a redshifted galaxy at its rest-frame emission peak wavelength of $100\text{ }\mu\text{m}$, an observation must be made at $200\text{ }\mu\text{m}$ where the beam size (FWHM) of the 20-m LDR is 4 arcsec. To make an unconfused measurement of an individual object requires a minimum separation between objects of about five beam widths. Thus, an LDR smaller than 20 m will be confusion limited by cluster galaxies and be unable to make the necessary observations.

Evolutionary effects in the rate of star formation become clear for galaxies at redshifts of $z = 1$ or greater, corresponding to eras 10 billion years ago. For closer, older galaxies, these effects are much less pronounced. IRAS showed that a typical spiral galaxy in the Hercules cluster ($z = 0.04$) emits about 50 mJy at $60\text{ }\mu\text{m}$. At a redshift of 1, such an object would produce about 160 μJy at $120\text{ }\mu\text{m}$, which would be detectable in about 1 hour with a signal-to-noise ratio of 10 with the 20-m LDR. Observing such galaxies with a smaller LDR would quickly become an impractical task, requiring days of integration time.

Perhaps the strongest spectral line from the warm interstellar gas found in spiral galaxies is the $158\text{-}\mu\text{m}$ transition of singly ionized carbon, CII. Our own galaxy emits 0.1 percent of its entire luminosity in this transition (Stacey et al., 1985). The $158\text{-}\mu\text{m}$ CII line can be used to characterize the thermodynamics of the interstellar medium and to give the total mass of the galaxy. Using the LDR, the CII line can be used to determine the properties of spiral galaxies out to the edge of the known universe. At $z = 1$ a galaxy like our own could be measured with a signal-to-noise ratio of 5 in 1 hour of integration time with 30 km/s spectral resolution using a 20-m aperture. Because of confusion with other cluster members, the small beam size of the 20-m LDR (3 arcsec at $300\text{ }\mu\text{m}$) is crucial for this program. Once calibrated by the study of relatively nearby galaxies, the CII line can be used to

measure the metric of the universe itself using the Fisher-Tully relationship, which relates to the width of the CII line and the total luminosity of the galaxy.

To date, there is no clear observational evidence for the existence of evolved planets orbiting stars other than our sun. While IRAS has detected shells of dust and debris orbiting nearby stars such as Vega and Beta Pictoris, it lacked the high sensitivity and spatial resolution to detect the thermal emission from a single Jupiter-like planet. The detection of planets around a statistically significant number of nearby stars would be possible with a 20-m LDR. It would quantify the probability of planet formation, and thus greatly constrain the theories of planet formation. In addition, for the first time, the number of planets in the galaxy could be accurately estimated, a number of great significance in the search for extraterrestrial life.

The direct detection of the thermal emission at two wavelengths from extra-solar planets yields the temperature of the planet, and thus its size can be estimated. Long-term observations yield the orbital period of the planet, which will determine the distance between planet and star. The temperature and distance can be combined to determine the effective emissivity of the planet and infer the existence of atmospheric effects or internal heat sources.

The direct detection of Jupiter-like planets drives the technology requirements of the LDR. The distance to which a 100-K Jupiter-sized planet can be detected in 3-hours' integration time in a broadband observation with a 20-m LDR at 30 μm (the peak of the thermal emission) is approximately 4 pc (and is directly proportional to the telescope diameter). There are 26 stars within 4 pc. Thus, a 20-m LDR is required for a statistically significant search. Assuming diffraction-limited angular resolution at 30 μm , and assuming that the near sidelobes (~ 3 -10 arcsec) can be kept down 40 dB (the planet is typically 10^3 - 10^4 times fainter than the star at 30 μm), the LDR will spatially separate Jupiter-sized planets that lie ≥ 10 AU from these nearby stars. Degradation of angular resolution (either by reducing the diameter of the telescope, by relaxing the diffraction limit at 30 μm , or by increasing the near

sidelobes) would increase this minimum separation, so that a planetary system like our own solar system would then become undetectable.

B. THE INTERFEROMETER OPTION

The LDR, from its inception, has been thought of in terms of a single circular-filled aperture. Yet, alternatives must be considered. The clearest alternative is one that has been chosen by many ground-based radio and millimeter-wave facilities: an interferometer array consisting of separate telescopes and generating interference data by means of delay lines and correlators operating at radio frequencies. Michelson interferometry, though briefly considered by the SCG, was considered impractical.

The issues are several, including science capability, technology limitations, and mission design. Interferometry offers improved spatial resolution, though not for all types of observation. Interferometers are complex, so that a meaningful comparison with a filled-aperture instrument must identify what can be accomplished at fixed cost. If radio techniques are used, then each interferometer element must be equipped with heterodyne instruments, and there must be a central facility to provide for appropriate delays and for multichannel spectral correlators. Finally, the added scientific capability cannot be considered in isolation. The LDR has a considerable set of scientific goals and corresponding science instruments. Telescope scheduling among competing instruments is an issue new to orbiting observatories, but one which must be met head-on for a complex observatory such as the LDR.

The advantage in a multielement array, as opposed to a two-element interferometer, is in quickly reconstructing an image and in the use of "phase closure" for elements whether they be free-flying, tethered, or rigidly connected. The signals would have to be correlated in space, as opposed to the concept of mixing in space at each of the elements and then transmitting the infrared to Earth to be correlated. The latter is impractical in terms of the required bandwidth, far exceeding the projected capability, for example, of the Tracking and Data Relay Satellite (TDRS).

A major concern for the LDR heterodyne interferometer would be instrument costs. The estimated cost of the total instrument package for the 20-m LDR is about \$100 million for each of eight instruments in the same range per instrument for those on SIRTf and ST. The cost is higher with the heterodyne interferometer since duplicate instruments for each element are required.

Another technical issue for a space-based array is to know, from moment to moment, the array geometry to a precision of a fraction of an operating wavelength. Earth-based interferometers assume the Earth to be perfectly rigid over the course of an observation, and they characterize the interferometer through a series of calibrating observations. In space, large "rigid" or tethered structures seem difficult to achieve and might introduce large-amplitude vibrations. One approach is to accept a free-flying array in which the relative separations of the elements vary smoothly according to their slightly differing orbits. An orbit analysis indicates that acceptable amounts of propellant could serve to cover the ultraviolet plane. Inter-telescope spacings might be measured by laser ranging, but the needed angular precision is several milliarcseconds, a daunting technical challenge. Use of an ultra-accurate star catalog such as that projected from Hipparcos might help for the case of free-flying elements, but must be considered speculative at this time.

Three specific configurations of comparable estimated cost were considered by the SCG in its study of space-based interferometry: (1) the fully instrumented 20-m filled aperture LDR, (2) an array of nine 4-m antennas (the "array"), assembled on a structure of 200-m extent and containing only single-element heterodyne mixers, and (3) a hybrid mission (the "hybrid"), consisting of a central 12-m element with full instrumentation and four 4-m elements with heterodyne mixers only.

1. The Array

An excellent discussion of some science goals that might apply to the array is presented in a report of the Submillimeter Telescope Committee of the Smithsonian Astrophysical Observatory. Although the wavelength coverage

of an LDR interferometer would differ from that in the ground-based instrument under study, many of the science goals would be similar. Largely, this similarity, which does not apply to the bulk of LDR science, results from the assumption of fixed-cost alternatives and, thus, from the limited instrumentation available to the array. Most of the LDR instruments would not lend themselves to interferometry. Bulk detectors and grating or Fabry-Perot spectrometers are not usable; neither are heterodyne arrays, chiefly because of data-handling problems. Even for single heterodyne mixers, the spectral coverage would have to be limited to fewer spectral channels. A 100-channel spectrometer with 36 interferometer baselines presents a formidable challenge for space-based data management. The conclusion of the SCG was that the science goals addressable by the array made it unacceptable as an alternative to the LDR, especially in the context of a millimeter/submillimeter interferometer construction boom. Most of the LDR-unique science objectives are not met by the array. The small antennas and the focus on longer LDR wavelengths do not sufficiently distinguish the science goals of a space-based array from those of ground-based interferometers. This situation might change after successful exploratory observations of a single-element LDR.

2. The Hybrid

The third, or hybrid, configuration for an LDR interferometer represented a compromise solution which would provide limited interferometer capability with a minimum impact on the many LDR-unique science objectives. It consists of a central, fully instrumented 12-m reflector and four 4-m elements with only single-element heterodyne mixers. The hybrid adds interferometer capability while preserving much LDR-unique science. However, the SCG felt that the penalties of operating such an unbalanced heterodyne array outweighed its advantages.

The first and most serious shortcoming of the hybrid is that it makes an inferior interferometer as compared to an array of identical antennas: (1) only four of the ten available interferometer baselines would include the central antenna, which contains over 3/4 of the total collecting area, (2) the field of view of the interferometer is restricted by its largest element,

whereas (3) the sensitivity on any interferometer baseline is equal to the geometric mean of the antenna diameters, more like that of the smaller element. Thus, for the hybrid, mapping would take 2.5 times longer than for a five-element array of equivalent total antenna collecting area, because of the restricted baseline coverage. The field of view, restricted by the control element, is only 11 percent of that in the array, restricting the number of pixels recoverable from a synthesized map. Finally, the large gain of the central element is not achievable on most baselines in the hybrid, restricting the detectable flux on these baselines.

The hybrid also encounters two other difficulties in comparison to the LDR. These have to do with mission design and with technology development problems. The central element of the hybrid is assumed to contain the full instrument complement. The interferometer elements contain only single heterodyne mixers. Even with the LDR, it was felt that eight instruments were possibly too much to mount at one time. In large part, this decision came about because of telescope scheduling problems; the realization that the observing needs of each instrument could only be efficiently met 12 percent of the time. The interferometer would make this problem worse. Full aperture synthesis is necessary for the complex source structures in the interstellar medium. This has been demonstrated by experiences with the OVRO and the Hat Creek interferometers. Such observations are slow. It would not make sense to build a hybrid, only to use it 20 percent of the time or less. Yet there are other pressing demands for the central LDR. In practice, it was felt that maximum scientific gain would result by using the four elements of the hybrid by themselves, continuously, and with a large field of view. But in that case, there is no reason to consider the four elements to be part of the LDR project. Thus, the hybrid, when faced with a real mission design, seems to divide itself into two separate projects: the LDR, and a later, separate array.

The final consideration which must be given to the hybrid is that of needed technology. The hybrid contains a large central reflector in addition to the full LDR instrument complement. That recreates all of the instrument and cryogenic needs, in addition to most of the controls and optics problems. In addition, the hybrid has all of the special needs of the array: multi-channel, multibaseline correlators on a limited power and weight budget and

the problems of maintaining and measuring the interferometer baselines in the absence of a solid foundation (e.g., the Earth).

3. Interferometer Science Discussion

The science goals for the LDR were reviewed in light of the capabilities of the LDR, the array, and the hybrid. It is useful to keep the sensitivity calculations in mind. For the array, we assume $N = 9$ antennas of $d = 4$ -m diameter each, with a maximum separation of $D = 200$ m. To be conservative we take a heterodyne detector bandwidth of $\Delta\nu = 2$ GHz at a wavelength of $100\text{ }\mu\text{m} = 3000$ GHz. A system temperature of $T_{\text{sys}} = 1000$ K and an integration time of $\Delta t = 4$ hours are assumed. The $1\text{-}\sigma$ rms continuum detection limit is then about $T_{\text{source}} \sim 100$ mK. The result scales as

$$T_{\text{source}} \sim \frac{D^2 T_{\text{sys}} d^{-2}}{\sqrt{\Delta\nu \Delta t N(N-1)}}$$

The angular resolution for the example is $\theta \sim 0.1$ arcsec and is proportional to D^{-1} . In other words, a dust map of a distant galaxy could be obtained at 0.1-arcsec resolution in 4 hours, to a 100-mK level. Unfortunately, this is comparable to or above the expected level of source temperature for most distant galaxies (see below). The hybrid sensitivity is more complicated, since not all baselines have the same gain. However, the numbers are similar. A preliminary cut was taken at the science, which is summarized below. We assumed the hybrid configuration in order not to sacrifice the bulk of LDR science, achieved with the full strawman instrument complement. The science was approached with the question "How much do we give up going from 20 to 12 m, and how much do we gain with the interferometer?"

a. Cosmology. Anisotropies in the cosmic microwave background at $\lambda \sim 1$ mm (including the Sunyaev-Zeldovich effect) have interesting scales at 10 arcsec to 1 arcmin. Thus, a 12-m telescope is nearly as good as a 20-m telescope. Counting highly redshifted dusty galaxies at $\lambda \sim 300\text{ }\mu\text{m}$ might require $\lesssim 3$ -arcsec resolution; here a 20-m dish is marginally better, but this experiment is speculative.

b. High Z Galaxies. The optical depth of dust through the disk of a spiral galaxy is $\tau_d \sim 10^{-2}$ at $\lambda \sim 100 \mu\text{m}$. Since the dust temperature is $T_d \approx 10 \text{ K}$, the expected brightness temperature is of order $T_b = T \cdot \tau \sim 0.1 \text{ K}$, not including a Z correction, which can be compared to the above equivalent noise temperature. The tentative conclusion is that dust emission would be too weak to map at high spatial resolution. The optical depth of the CII (158- μm) transition through a spiral galaxy is also of order $\tau \sim 10^{-2}$. The excitation temperature of CII may be as high as 100 K, but the decrease in $\Delta\nu$ for this line makes the noise temperature ~ 30 times higher than in the case for dust emission so this line would also be hard, or impossible, to map at 0.1 arcsec. Interferometry seems to offer little here.

c. Nuclei of Galaxies. QSOs have jets whose size scales range from subparsec to many kiloparsecs. The central engines may have size scales of order astronomical units. Thus, the more resolution the better (1 pc = 0.01 arcsec at 20 Mpc, a "typical" distance to a nearby active galactic nucleus). In our own galactic center, we see very interesting ionized clouds and molecular rings with size scales of order 0.1-1 pc. To look at nearby galaxies to the same spatial scale requires 0.01-arcsec angular resolution. Such resolution may help separate thermal and nonthermal components of the continuum. These observations do not require high sensitivity, but high spatial resolution. Line emission from the fine structure transition of, for example, OIII, CIII, and OI may also be detectable (these will allow velocity measurements, hence the dynamics of the region).

d. Galaxies. A 20-m LDR would do a great deal in looking at global star formation, galactic structure, etc. The questions are "How do giant molecular clouds form?" and "What is the thermal history of interstellar gas?" It may be that a 1-arcsec beam observing nearby galaxies will be enough to answer these questions. No strong arguments for interferometers were unearthed here.

e. The Center of Our Galaxy. The spatial resolution of a 20-m dish is sufficient and its large gain is desirable. The ionized and neutral clouds have size scales $\gtrsim 1$ arcsec.

f. The Structure of Our Galaxy. Most structure questions are answered at resolutions ≥ 1 arcsec; thus, a 20-m dish is needed.

g. Chemistry and Interstellar Processes in Our Galaxy. Some projects would be helped by better resolution: (1) searching for molecules with no electric dipole moment like O_2 and CH_4 but which have induced-dipole transitions in the submillimeter and infrared, (2) probing the density peaks in molecular cores for exotic molecules (here the "spatial filtering" helps to untangle the "forest" of lines which will probably be observed), (3) observing at high spectral resolution (0.1 km s^{-1}) and spatial resolution the effects of ambipolar diffusion (different molecules have different slip velocities) and thereby estimating magnetic field strengths, and (4) measuring polarization from the molecular lines.

h. Star Formation. Protostars are thought to have disks around them which form planets. The size of these disks are ~ 10 AU. At the distance to the nearest star-forming region (in Taurus), this corresponds to 0.1 arcsec. To resolve these disks in order to look for condensations which may form planets, to study the accretion shock on the disk (possibly observable through the accreting cloud at $\lambda > 100 \mu\text{m}$), and to really "see" the basic processes of star and planet formation require an angular resolution of 0.01 arcsec (preferably) to 0.1 arcsec. The continuum from the disk is observable with the interferometer at 0.01-arcsec resolution. A calculation needs to be done on the observability of the accretion shock lines. Ideally, we would like to see line emission in order to sort out the dynamics at ≤ 100 AU. These observations represent some of the strongest arguments for interferometric capabilities on the LDR.

i. Outflows. Outflows were discussed in terms of protostellar outflows, as well as outflows from old stars such as red giants, novae, WR stars, Miras, etc. In the former, it appears that the flows are collimated by ≤ 10 -AU disks, and that the flows are clumpy at scales < 1 arcsec and probably < 0.1 arcsec in the nearby regions (Taurus, Orion). H_2O masers are clumps with size scale 10^{14} - 10^{15} cm, which is < 0.1 arcsec at Orion. Around old stars, the great problem is that of dust formation--How does dust form? Does it form in clumps? Here again, 0.1-arcsec resolution is needed and 0.01 arcsec is

preferable. A first guess is that all these observations are not sensitivity limited, rather they require angular resolution.

C. THE LIGHT BUCKET

The science report of the 1982 Asilomar workshop on the LDR suggested the use of a 20-m diameter LDR as a light bucket at wavelengths $\lambda < \lambda_d$, where $\lambda_d = 30$ to $50 \mu\text{m}$ is the shortest wavelength to which the telescope is diffraction limited. In particular, it stressed the application of the LDR to the problem of observing distant high redshift galaxies to the fundamental limit that the galaxy images overlap (the "confusion limit"). In this application, a passively cooled mirror temperature $T_{\text{LDR}} < 200 \text{ K}$ was important. In addition, the report compared the LDR with other telescopes in the $1\text{-}\mu\text{m} < \lambda < \lambda_d$ wavelength range.

Several points have emerged in the last few years concerning light bucket operation of the LDR, which represent new findings or different conclusions from the earlier Asilomar report. Because of the likely development of much better detector sensitivity than was assumed in the Asilomar report, SIRTf gains in advantage over the LDR for low spectral resolution work at $\lambda > 4 \mu\text{m}$ (e.g., distant galaxies). At the same time, the large ground-based telescopes will be able to reach the confusion limit of distant galaxies for $1 \mu\text{m} < \lambda < 2.5 \mu\text{m}$, although with considerably larger integration times than the LDR. Consequently, the science rationale de-emphasizes the use of the LDR as a photometer between $1\text{-}4 \mu\text{m}$, but emphasizes more the use for high resolution spectroscopic mapping in the $1\text{-}30 \mu\text{m}$ range. Good light bucket capabilities, if not diffraction limited performance, are very desirable at $28 \mu\text{m}$ in order to probe the distribution of H_2 . Subjectively, the $\lambda < 25\text{-}\mu\text{m}$ light bucket science, although interesting, does not seem to warrant a major increase in the cost of the LDR.

The cost of achieving a ~ 1 arcsec light bucket capability for $1 \mu\text{m} < \lambda < 25 \mu\text{m}$ is uncertain at present. Each 2-m segment must individually achieve a spot size of ~ 1 arcsec; alignment of the spots from each segment to within ~ 1 arcsec will automatically be achieved if the telescope is diffraction limited at $30\text{-}50 \mu\text{m}$. The increased cost of producing optical

quality 2-m segments vs 30- μ m diffraction-limited segments has yet to be determined. The importance of passively cooling the telescope is de-emphasized. The pixel size ($\geq 100 \mu$ m) for a 1-2 arcsec light bucket may be so large as to degrade detector performance.

In light of the above scientific and technical considerations, we suggest a "goal" that the LDR have the "light bucket capability" to focus 50 percent of the $1 \mu\text{m} < \lambda < 30 \mu\text{m}$ radiation from a distant point source into an image diameter $\theta_{\text{LB}} \leq 1$ arcsec if the increased cost on the LDR system to achieve this capability does not exceed 20-30 percent of the total cost and if no major delays in the LDR program are affected.

D. INSTRUMENTS AND CRYOGENS

The SCG subgroup on instruments provided a strawman focal-plane configuration (described in Section I of this publication and in JPL Internal Document D-2214). Arguments and calculations were provided to indicate what instruments would be required to satisfy the science goals of the LDR and how the instrument program should be implemented. Both the current status and development needs were described in JPL Internal Document D-2214. A summary of the document is provided here.

1. Instruments

The astronomical projects outlined in the Asilomar reports require that the LDR be equipped with a combination of instruments capable of observing line profiles ranging in intrinsic velocity width from a fraction of a kilometer per second to several hundred kilometers per second, as well as broadband cameras capable of using the full spatial resolution of the 20-m telescope. The means by which this range of resolutions is achieved must be consistent with the primary goal: sensitivity which approaches as closely as possible the fundamental limits, over the entire wavelength range of the LDR. By way of introduction to the "strawman" package of instruments, we first discuss in general terms the available types of detectors and detection schemes and provide a comparison of their performances. Because the LDR is a warm telescope ($T = T_{\text{ambient}} \sim 200 \text{ K}$), the noise sources which limit

sensitivity are mostly the same ones encountered by infrared instruments on present ground-based and airborne telescopes, and we find that the experience with these systems serves as a useful guide in the definition of the LDR focal-plane instruments. The analysis may be summarized as follows. For all wavelengths at high resolution ($\Delta\nu/\nu \lesssim 10^{-5}$), heterodyne receivers are the only practical instruments. At lower resolutions, the most successful mid- and far-infrared instruments are narrow-band spectrometers, based on Fabry-Perot interferometers or diffraction gratings and photoconductive detectors. The superiority of these spectrometers derives from the fact that shot noise in the photons emitted thermally by warm optical elements dominates the system noise (this condition is often referred to as background-limited performance). Finally, for broadband photometry, cameras based on large area arrays of photoconductors would be used except at long wavelengths ($\lambda \gtrsim 200 \mu\text{m}$), where they would be supplanted by smaller arrays of bolometers.

From the arguments in the full report it appears that there could be as many as eight instruments in the focal plane of the LDR. This instrument complement is summarized in Table 3-1 and Figure 3-1. It is anticipated that technological developments will make redundant or replace some of the proposed instruments. However, the strawman list appears to be comprehensive at this time since a detection system is provided to address each of the scientific goals.

2. Cryogenics

There are many factors that combine to set the requirements for the cryogenics system for the LDR focal plane. Unfortunately, at this time, some of them cannot be defined. For example, a primary factor is the lifetime of the coolant system before replenishing or servicing. The overall lifetime of the LDR is assumed to be in excess of 10 years; however, this could be impractical as a lifetime for some of the possible cooling schemes, e.g., stored liquid cryogens. The time between services will depend on the orbit, the availability of the Shuttle, and the status and nature of the Space Platform. Clearly, it might be partially catastrophic for the LDR to allow the instruments to warm up, so that a conservative estimate must be made for the time between services. Three years seems to be a reasonable time to assume for the interval between service of some kind. A second critical factor is the nature of the instrument in-orbit

Table 3-1. LDR Strawman Instrument Complement

Number	Instrument	Type	Wavelengths
1	High-resolution spectrometer	SIS multichannel heterodyne receiver	3 mm-400 μm
2	High-resolution spectrometer	Schottky diode multichannel heterodyne receiver	500-200 μm
3	High-resolution spectrometer	Photoconductor multichannel heterodyne receiver	200-35 μm
4	Medium-resolution spectrometer	Fabry-Perot interferometers with imaging detector arrays	200-35 μm
5	Medium- to low-resolution spectrometer	Multichannel grating spectrometers	200-35 μm
6	Heterodyne array	SIS array	?
7	Far-infrared camera	Photoconductor arrays, broadband filters, interference filters	200-35 μm (5-1 μm) ¹
8	Submillimeter camera	Bolometer arrays, broadband filters, interference filters, FTS?	1 mm-100 μm

¹The short-wavelength camera may best be supported in the on-axis guider.

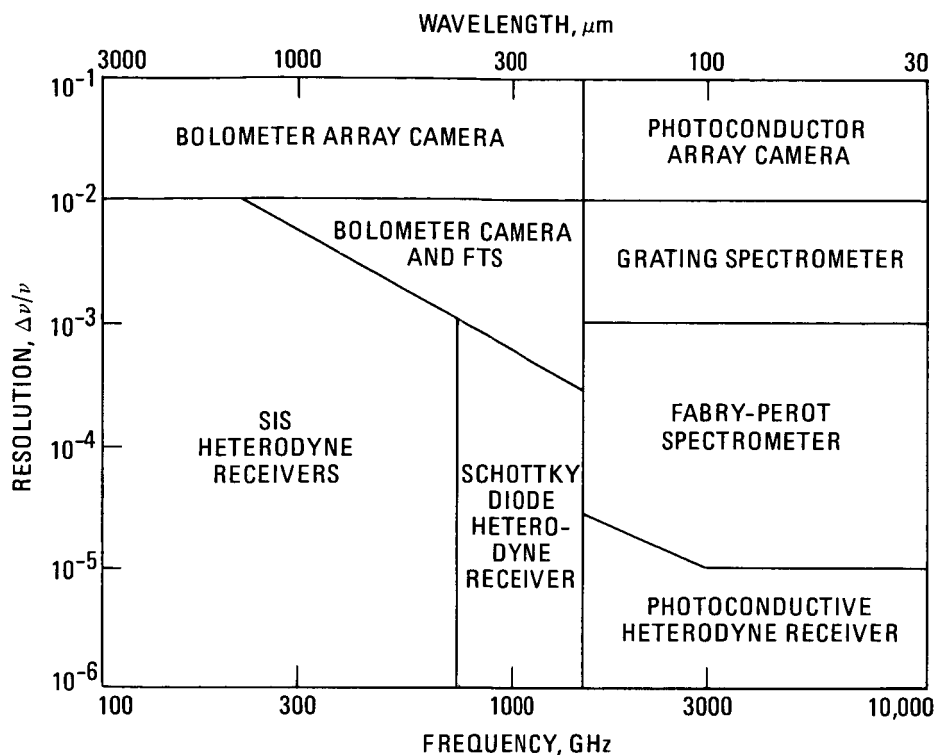


Figure 3-1. Range of resolution and frequency covered by the LDR strawman instruments

changeout requirement. It may be that some instruments will fail, be superseded technologically, or be replaced for astronomical reasons. An important aspect of the proposed scheme for the structure of the instrument package is the provision for in-orbit instrument changeout. The cryogenic system must be able to cope with individual supplies of cryogenics to each instrument package, and an automatic disconnect and shutoff system must be incorporated. A factor which is reasonably straightforward to define is the heat load. This depends on the nature of the telescope and the instruments but, with our assumptions about these stated above, an estimate can be made. The resulting basic requirements for the cooler are

- (1) Lifetime between services ~3 years
- (2) Separate disconnectable supply of cryogenics to each instrument
- (3) Cooling load 3.5 W at 20 K; 1 W at $\lesssim 4$ K

Needless to say, a special system must be developed to meet these requirements. The candidates appear to be stored liquid helium (IRAS technology), which seems difficult due to the lifetime and independent cooling requirements; a closed-cycle mechanical cooler, combined with a Joule-Thomson (J-T) stage--possibly inside each instrument; closed-cycle cascaded Joule-Thomson systems; and adsorption refrigerators. It should be noted that some refrigeration at $T = 2-3$ K is required for the long-wavelength germanium detectors, a range which is difficult to reach with mechanical and J-T refrigerators.

It is important to proceed with a technology study of this issue since it is critical to the viability of the LDR. The capabilities of the refrigeration scheme will influence the design of the instruments and may provide a significant electrical power load to the spacecraft itself.

3. Generic Electronics

The heterodyne instruments and, to a lesser extent, the spectrometer and camera arrays require a sophisticated back-end electronics package. Since the major problem is that of the heterodyne instruments, we will deal with their requirements in some detail.

Each heterodyne instrument may have up to about 10 GHz of intermediate frequency (IF) bandpass, within which spectral resolution may be required down to 1 MHz (corresponding to 0.25 km/s at 250 μ m) or less in certain situations. Thus, there will be $\sim 10^4$ simultaneous data channels. Various techniques may be applied to this problem, including

- (1) Banks of analog filters
- (2) Digital autocorrelators
- (3) Acousto-optical spectrometers (AOS)
- (4) Some hybrid combination of these

A bank of analog filters would probably be insufficiently flexible to satisfy the wide range of needs on the LDR. The two strongest candidates would be the AOS and digital autocorrelator spectrometers. However, a hybrid digital autocorrelator/filter system also appears feasible and may be advan-

tageous in terms of power consumption. A brief description of such a hybrid system is given for an example. However, some development of AOS devices is considered important for obtaining broadest spectral coverage.

It can be shown that the chip count and power requirements of a digital autocorrelation spectrometer are reduced by a factor of N if the total bandwidth to be analyzed, B , is first prefiltered into N bands, each of width $b = B/N$. For a spectrometer used for submillimeter-wave astronomy, a value of $B \sim 4000$ MHz is appropriate, and the prefiltering is most efficiently realized with analog filters operating at ultrahigh frequency center frequencies and utilizing either surface-acoustic-wave (SAW) or hybrid integrated circuit techniques. The choice of N is a trade-off between amounts of analog and high-speed digital equipment in the system; N equal to 512 results in individual correlators covering a modest 8-MHz bandwidth, for example.

E. ANCILLARY REQUIREMENTS

1. Beam Stability

The requirements for a precise stable LDR are very demanding. One requirement for surface accuracy of the LDR mirror is determined by the shortest operating wavelength. However, there is a requirement that the beam shape, particularly the levels and positions of the first few sidelobes, must not change during an observation and before the beam shape is calibrated on a bright point source. Moreover, some classes of LDR science will require long multiorbit integration. Assurance of a rapid return to a stable, calibratable beam pattern, following sun/Earth/moon avoidance slews is essential. Variations in the beam profile determine the dynamic range of the resulting map, i.e., the ratio of the strongest contour to the weakest believable contour of the map. This is often a more important quantity than simple sensitivity or resolution in determining whether a project is feasible or not. Much of the science which the LDR is intended to do demands high dynamic range; the search for planets around nearby stars sets the most stringent requirements.

Examples of observations that require high dynamic range are common in both the continuum and spectral line modes, including continuum mapping of the

internal structure of a source and detecting faint companions of a bright source. The search for planets near stars, jets near active galaxies, gravitational lens images of quasars, structure in planetary atmosphere, and mass loss from T Tauri stars are all continuum projects that will require a dynamic range of 250:1 to 1000:1.

In spectral line work the goal is to be able to measure independent spectra from adjacent beam areas, with "leakage" of strong nearby lines in adjacent pixels. This is required for dynamical maps of clouds or galaxies with large variations in brightness, to separate halos from point sources, or to measure large spectral differences between two nearby sources (e.g., in absorption). An example of this is the measurement of galaxy rotation curves. Historically, there was intense controversy in radio studies of the 21-cm line in galaxies over flat rotation curves, because single-disk telescopes like Arecibo could not achieve a dynamic range better than $\sim 50:1$. Leakage of the spectrum from bright internal portions of galaxies into beams much farther out provides false signature of the flat rotation curves which characterize massive halos. Interferometer observations were needed to confirm the true nature of the rotation curves and to establish the presence of halos unambiguously.

The critical telescope quality which determines the sidelobe stability is not simply surface accuracy, but large-scale distortion of the dish. Typically, if the second or third sidelobes (which are already factors of 20 to 200 down from the main beam response) change by more than 3 to 5 percent during an observation, or between a calibration and the following observation, then the dynamic range of the map will be degraded. Critical scales of the distortion which count are generally 0.01 to 0.3 of the area of the dish, corresponding to individual panels or groups of panels. A single cocked panel can look like a faint stellar companion. If regions of this size deviate from the true figure by $\delta/20$, then the beam pattern will change. A workable specification is to keep the point spread function stable over two orbits to within 5 percent of the envelope of the sidelobes at each point with 20 half-power beam widths of the center of the main lobe.

2. Space Station and LDR

Three different options have been considered for placing the LDR in orbit using the Space Station. The three options are described below.

a. Co-orbiting Platform Option. The LDR will be mounted on the co-orbiting platform. Services such as power, telemetry, cryogenics, pointing system, etc., will be platform-supplied. Periodic servicing will be required. The LDR will be initially brought to orbit by one or more Shuttles and assembled and tested at the Space Station. Operation will be from a remote ground station.

b. Space-Station-Assembled, Free-Flyer Option. The LDR will be brought to the Space Station by one or more Shuttles. Assembly and test will be carried out at the Space Station. The LDR will then be launched as a free-flyer. All resources such as power, telemetry, etc., are payload supplied. Periodic servicing will be required by use of an orbital transfer vehicle. Operation will be from a remote ground station.

For the LDR to realize its full potential, it may have to be assembled in orbit from multiple Shuttle loads. The Space Station is essential to this assembly process. It is only in the case where the LDR can be deployed autonomously from a single Shuttle launch that the Space Station is not really required.

c. Space-Station-Attached Option. The LDR will be brought to the Space Station by one or more Shuttles. Assembly, test, and permanent mounting will be on the Space Station. Services such as power, telemetry, cryogenics, and pointing system will be Space Station supplied. Periodic servicing, instrument changes, and some operations will be performed by the Space Station. Most operations will be from a ground station.

Table 3-2 lists the major advantages and disadvantages of three options as they affect the LDR. In addition, a free-flyer, not associated with the Space Station, is included for comparison.

Table 3-2. Major Advantages and Disadvantages of LDR Options

Option	Advantage	Disadvantage
Co-orbiting platform	Easily served	Polar orbit desirable for maximum thermal stability not possible
	Spacecraft services provided	
Free-flyer Space Station-assembled	Polar orbit possible	Difficult to service
	Clean, quiet environment	Difficult to repair
	Multiple Shuttle loads can be assembled in orbit	Must provide all services
Space-Station-attached	Very easily serviced	Polar orbit not possible
	Real-time operation from Space Station	Contamination and vibration problems
	Easily repaired	Field of view may be limited
	Spacecraft services provided	RFI problems
Free-flyer not associated with Space Station	Polar orbit possible	Difficult to assemble
	Clean, quiet environment	Multiple shuttle launches unlikely
		Difficult to service
		Difficult to repair
		Must provide all services

A major advantage of the Space-Station-attached or co-orbiting modes is the potential for instrument substitution, adjustment, and repair. This versatility will be critical if the LDR is to fulfill its mission as a general-purpose facility for far-infrared astronomy. The problem is that there is so much new science to do that it is difficult to prioritize. The wavelength range is so broad (30 to 1000 μm) and the choice of spectral resolutions so wide that it is hard to imagine how to cover the whole without periodic receiver changes. The scientific goals of the observatory extend from solar system studies through galactic and extragalactic astronomy to cosmology; hundreds of projects are envisioned, each with different criteria. Spectral lines of important atoms and molecules are spread throughout the band. Although multiple feeds and multiple receiver systems are possible, it is hard to imagine a single package that will allow all receiver and resolution combinations. The increase in capabilities of the LDR makes a narrow choice of configurations dangerous. The Space Station completely changes this situation. The ability to change receivers, or to change spectrometers on a given receiver, or to change dewars, or otherwise to reconfigure the system with major replacement of components, makes the goal of a general-purpose instrument feasible. If periodic service were available from the Space Station, then all sorts of observations could be accommodated which cannot be provided for if we preordain the instrumentation, as we must for a free-flyer without the Space Station capabilities. A modular design of dewars, receivers, and spectrometers would allow quick changes. It may be that after 1 to 2 years of "shakedown" the LDR could recede from tethered mode to a co-orbiting position, visited thereafter only once or twice a year. The possibility of periodic visits for reconfiguration of the receiver changes all the problems associated with the design and specification of receivers.

The minor disadvantages of a Space-Station-attached LDR are pollution (contamination of cold surfaces by condensation of volatiles emitted by the Space Station or the Shuttle), oxygen erosion, radio frequency interference, and simple blockage. Since the LDR must point and repoint frequently over large angles, always avoiding the sun and Earth limb by approximately 45 deg, the Space Station may severely limit LDR pointing.

To avoid the contamination and oxygen erosion problems at the Space Station orbit, it is now thought best to use the Space Station for assembly

and checkout of the LDR and then boost the LDR to a ≥ 700 -km orbit with the orbital maneuvering vehicle (OMV). Servicing and instrument replacement can be accomplished with the OMV, however, with somewhat more difficulty than at the Space Station.

3. Telemetry

Two types of experiments will place heavy loads on the telemetry of the LDR: driven scans for mapping and heterodyne spectrometry, which will in some cases require the intermediate frequency to be transmitted to Earth for processing. A typical case of driven scans will be mapping of the $158\text{-}\mu\text{m}$ CII line, which is strong enough to give good signal-to-noise ratio in intermediate- to high-resolution spectrometers in a time as short as the drive time across one beam size at a drive rate of 1 deg per minute or faster. If we assume a maximum scan rate of 1 deg per minute, then we will want to transmit 300 spectra per second to fully sample. For 1000 spectral channels, this requires 3×10^5 intensity values per second. For 16 bits per member, that makes 5×10^6 bits per second. If the single spectrometer element were replaced by a linear array of 100 elements, then the rate goes up to 500 Mb.

A heterodyne mixer operating at $100\text{ }\mu\text{m}$ with total bandwidth of 250 km/s (good for galactic work) would need 2.5 Gbaud to transmit the predetected signal to the ground for further processing. This is an important capability for the LDR to have, although it may be used only about 10 percent of the total observing time.

4. Chopping, Thermal Control, and Figure Control

The LDR is a warm telescope, and throughout its prime wavelength band of $30\text{--}300\text{ }\mu\text{m}$ the vast majority of the photons reaching the focal plane will come from the telescope instead of astronomical sources. The measurement of small astronomical signals in the presence of the large telescope background requires careful design and attention to small details. The usual technique of modulating the signal with a known pattern, followed by phase-sensitive demodulation, is still the best choice; the signal is modulated by using a chopper, by wobbling the entire telescope, or by frequency switching in a spectrometer.

The background as seen from a detector at the on-axis focal plane, within the cone with opening angle θ set by the system F/D ratio, should have an intensity I_λ

$$I_\lambda \leq B_{\max} = \epsilon B_\lambda(T)$$

where ϵ and T define an effective system emissivity and temperature, but any combination of mirror emissivities and temperatures that satisfy the limit is acceptable. The LDR total background specification is

$$\epsilon = 0.05$$

and

$$T = 150 \text{ K}$$

a. No Dark Ring. The background seen by a detector in angles 0.8-1.2 times θ should also be $\leq B_{\max}$.

b. Field Uniformity of Background. The background should be uniform within the telescope field of view to within 1 percent of B_{\max} .

c. Temporal Stability. The background should not vary rapidly with time when the chopper is held at a fixed angle. Within a typical chopping period τ_c the background must be stable enough that photon fluctuations dominate the noise. Since about 10^{12} photons per second hit the detector, the background must be stable to 1 part per million, but only on short time scales. The requirement is that the temporal variation of the background defined as

$$\Delta_t B = 0.25 [B(t) - B(t + \tau_c) - B(t + 2\tau_c) + B(t + 3\tau_c)]$$

should satisfy

$$\Delta_t B \leq 10^{-6} B_{\max}$$

for all values of τ_c between 0.1 and 1 s. This is effectively a second derivative of the background, so linear drifts do not penalize system performance.

d. Limit on Chopper Offset. The chopper should not modulate the background. Since observations of sources 10^8 times fainter than the background are planned, the chopper must cancel almost all the background to allow the later analysis stages to work with practical dynamic ranges. Thus, the chopper offset defined as

$$\Delta_c B = B(+ \text{ end of throw}) - B(- \text{ end of throw})$$

should satisfy

$$\Delta_c B \leq 10^{-5} B_{\max}$$

at all chopper throws up to the maximum specified throw. This is practical with well-designed chopping secondaries (as demonstrated by the performance of ground-based telescopes--e.g., the Infrared Telescope Facility (IRTF)).

e. Field Uniformity of Offset. The chopper offset should be uniform in the field of view of the telescope. Define the change with angle in the field of view of the chopper offset to be $\nabla_{\theta} \Delta_c B$. This should satisfy

$$\nabla_{\theta} \Delta_c B \leq 10^{-7} B_{\max}$$

This specification is 1 percent of the chopper offset.

f. Overall Photometric Error. Observations of sources will be made with the source in both the + and - beams of the chopper, in order to cancel the chopper offset. These observations will be separated in time by an interval τ_w that includes the time necessary to move the telescope by the maximum chopper throw and have it settle. The overall photometric error defined as

$$\nabla_w \Delta_c B = [\Delta_c B(t) - \Delta_c B(t + \tau_w) - \Delta_c B(t + 2\tau_w) + \Delta_c B(t + 3\tau_w)]$$

should satisfy

$$\Delta_w \Delta_c B \leq 10^{-8} \sqrt{\frac{2500}{\tau_w}}$$

where τ_w is defined as four times the time required for the telescope to move by the maximum chopper throw and settle into a stable observing condition. Note that the overall limit on second derivatives of the offset varies like the $-5/2$ power of the wobble time τ_w , so an agile telescope has a great advantage.

These limits are all achievable by telescopes with chopping secondaries and nonsegmented primaries. The SCG has written these specifications not to rule out any technically desirable designs, but rather to focus the attention of engineers and designers on a very serious issue for an infrared telescope that has no carryover from surveillance cameras or laser beam formers. Appendix C uses these specifications to derive requirements on mirror cleanliness and the mechanical stability of the primary mirror.

5. LDR Orbital Environment

a. Atmospheric Drag. The LDR will have a mass of 2×10^4 kg and a surface area of 800 m^2 , so the area-loading M/A will be about 25 kg/m^2 . The drag force will be about $F = A\rho v^2$, leading to an orbital decay rate of

$$\frac{dh}{dt} = \frac{2A\rho v^3}{Mg}$$

so if we allow 100 km in 10 years we need $\rho = 1.5 \times 10^{-18} \text{ gm/cm}^3$, which is reached at $h = 810 \text{ km}$. This is a very high altitude for large manned space operations, so the LDR would need to have propulsion for reaching operational altitude and for deboosting back to Shuttle altitudes for refurbishment. The OMV could provide this propulsion. However, if the LDR has boost built in, we should consider the option of lower orbits with periodic reboosts. The drag force on 800 m^2 at $h = 500 \text{ km}$ is only 3000 dynes. Thus, a fuel consumption of only 0.03 gm/s with exhaust velocity = 1 km/s will provide the required thrust, leading to a fuel requirement of 1000 kg/yr. At 300 km, the density is 40 times higher, requiring over 10^4 kg/yr, which is impractical. Thus, the LDR could fly with a Space Station, if it had a big enough engine.

b. Surface Erosion. The LDR will have to worry about surface erosion effects, but these will not be worse for the LDR than for other missions that fly sooner. The Space Telescope will fly at 500 km for example. If the LDR is at 810 km, the surface erosion effects will be negligible. But if the LDR flies with a Space Station at lower altitudes, then more care is needed. However, if the LDR is designed with a sunshade that keeps the sun and Earth light off of the main mirror, the same sunshade will keep the "wind" off the mirror, since the LDR will only be able to look about 67 deg from the zenith and will not ever look in the ram direction. An operational constraint to keep the line of sight (LOS) farther from the ram direction than this would cost some observing time and efficiency.

c. LDR Slewing and Observing Plans. If the LDR has targets densely populated over the sky, then increased slew rate will always increase observing efficiency, but slowly, while the power costs of rapid slewing rise rapidly. The efficiencies that can be achieved depend on the orbit inclination, altitude, and the sun-Earth avoidance constraints.

In the worst-case low inclination orbit, the sun can go right overhead. In this case the angle between the sun and the Earth limb is 112 deg, because the horizon is depressed by 22 deg from a 500-km altitude. These bad cases occur when the sun is in the orbit plane, so the angle between the Earth-sun line and the orbit plane, β , is 0 deg. β will oscillate with an amplitude of plus and minus the inclination at a period of about 38-64 days, and with an amplitude of ± 23 deg with a period of 1 year. Thus, the LDR will experience good times and bad times in a low inclination orbit, but for a 57-deg inclination orbit one can have $\text{abs}(\beta) > 15$ deg most of the time. In any case, the LDR will need a very good shade to operate in an Eastern Test Range (ETR) orbit--say 60-deg sun avoidance and 45-deg Earth limb avoidance, and operations would have to be curtailed during the $\beta = 0$ seasons.

In a sun-synchronous polar orbit, with a 97-deg inclination, β can be held to > 60 deg, so the angle between the sun and the Earth limb is at least 172 deg at all times. The operational requirements on the LDR are then quite relaxed and only require approximately one turn/orbit to stay away from the Earth. For 45-deg limb avoidance, the duration of a single pointing can be $(180 - 2 \times 45 + 2 \times 22) / 360$ of an orbit, or about 35 min.

Edward L. Wright has written a computer program that tries to schedule observations for the LDR, satisfying the sun and Earth constraints while maximizing the useful observations. This program assumes that useful data is obtained after a certain setup time following a slew. The attainable efficiency depends on the density of targets, and since the LDR will be a very powerful observatory, the following table is based on a large number of targets: 24 points at $b = 0$ deg, 8 points at $b = \pm 20$ deg, 24 points at $b = \pm 60$ deg, and 2 points at the galactic poles. Inclusion of high galactic latitude sources is necessary to get high efficiency, since the galactic plane is often entirely inaccessible. Leaving out the high latitude sources costs 8 percent in efficiency for a 57-deg, 500-km orbit with 20-deg/min slew rate. The program attempts to equalize the observing time given to various sources; if asked to be "more equal," the efficiency goes down. Using the above sources, and enforcing a modest equality of observation, the annual average efficiencies (Table 3-3) were obtained for 500-km orbits with a setup time of 5 min.

Since 15-20 min of each orbit are being used up by setup time, the efficiencies are close to the maximum allowed by the slew rate, which is $[(SLEW-3.8)/SLEW]$, where 3.8 deg/min is the orbital rate. Thus, avoiding the sun in a non-sun-synchronous orbit will not cost significant observing time, when averaged over a year. Further, slew rate requirements higher than 20 deg/min are not justified by this analysis; sunshade performance better than $\text{sun} > 60$ deg and Earth limb > 45 deg from the LOS is also not required. A surprising conclusion is that ETR orbits are quite good. The IRAS orbit is a few percent better, but this advantage is due to the 900-km altitude.

Table 3-3. Annual Average Efficiencies

Slew Rate, deg/min	Inclination, deg			
	57	23	97	IRAS (99 deg, 900 km)
10	0.51	0.51	0.52	0.57
20	0.66	0.66	0.66	0.71
30	0.71	--	--	--
50	0.75	0.74	0.74	0.78

The torque and power required to point the LDR are substantial. A 20-m LDR will have a moment of inertia $I = 10^7 \text{ kg-m}^2$, so with $\omega = 0.004 \text{ rad/s}$ (15 deg/min) the angular momentum is $L = 40,000 \text{ N} \cdot \text{m/s}$. Very large control moment gyros are the only practical way to maneuver the LDR.

The LDR will probably have a large difference between its moments of inertia on different axes. This will generate gravity gradient torques, following $T = (dg/dh)\Delta I$. For $\Delta I = 10^6 \text{ kg-m}^2$, this is $3 \text{ kg m}^2/\text{s}$, so the gravity gradient torques can build up an angular momentum equal to the slewing momentum in a few thousand seconds. Clearly the LDR will require some momentum management planning and a large magnetic torquing capability. A good mission planning routine should be able to keep the angular momentum under control.

SECTION IV

DEVELOPMENT PROGRAM LEADING TO THE LDR

The details of the LDR technology development program are given by Nishioka (1986) and Phillips and Watson (1984). The highlights are given below.

A. REFLECTOR PANEL DEVELOPMENT

Requirements for LDR mirror panels are (not necessarily in the order of importance) (1) satisfactory figure and surface quality, (2) light weight (low mass per unit area), (3) rapid fabrication, and (4) low cost. In particular, weight savings in the primary reflector surface (segments) are magnified by related savings in support structure and, consequently, in inertia of the system, thereby resulting in reduced control moment gyro (CMG) torque and momentum storage requirements, which all result in mass and electrical power savings. Furthermore, these savings have a ripple effect throughout the system, affecting nearly all subsystems and consequently reducing total cost. For example, if the system is weight constrained, the savings in the primary mirror mass could be translated into added cryogen which in turn would reduce the frequency of cryogen replenishment, i.e., servicing of the LDR. This reduction in servicing frequency is significant because servicing is expensive not only in terms of dollars, but also in telescope downtime and in risks associated with servicing. System trade-off studies should be carried out to quantify the relationships between savings in mirror mass and total system impact, including cost and risk reduction.

Candidate materials for the lightweight primary-mirror segments are (1) low-expansion glasses (such as fused SiO₂), (2) composites (such as graphite/polymers, graphite/glass, and carbon/carbon), or (3) Hexcel or foam aluminum-core structures. For each of these candidates, questions on their suitability remain to be answered, such as

- (1) Are ultralightweight glass panels (<20 kg/m) too fragile for LDR deployment or assembly?

- (2) What is the long-term dimensional stability of the graphite/polymer composite in the face of moisture changes or when exposed to high-energy radiation in space?
- (3) Are there possible contamination effects caused by outgassing of the polymer composites (postlaunch and STS or Space Station environment)?
- (4) What are the problems associated with aluminum sandwich panels (i.e., what effect does venting have during ascent, what effect does anisotropy with Hexcel cores have on the figure during manufacturing and maintenance/control, and what effect does the high-expansion coefficient of aluminum have when there is a great discrepancy between manufacturing and operating temperatures of the primary mirror and there is a large difference between the coefficient of thermal expansion (CTE) of the panels and the support truss)?
- (5) The issue of micrometeorite (and orbital debris) impacts on the LDR during its lifetime has been raised--would one segment material have advantages over the others in terms of effects of such impacts?

As a result of these unanswered questions, it is proposed that technology development of primary-mirror materials be focused to accomplish the following:

- (1) Determine if the ultralightweight glass fragility issue is real, and if so, determine if this problem will be adequately addressed by Department of Defense programs so that no significant funds need to be expended by the LDR. Acoustic/vibration and other mechanical tests (including remote manipulation as well as handling by astronauts) should be conducted on ultralightweight glass panels to resolve this issue.
- (2) Continue and expand the present development work on composite materials. Obtain data on anisotropy effects and on the long-term dimensional stability of composites and the effects of the space environment on that stability by accelerated-life test experiments, analysis, and comprehensive survey.

- (3) Test the performance of metal-core segments (Hexcel or foam) in a vacuum to determine whether the anisotropy of Hexcel structure is a significant issue of manufacturing or maintenance/control of the segment figure. Determine by analysis simulation, and experiment with the dimensional effects between the metal mirror segment and their (low-expansion graphite/xx) support structure.
- (4) Determine if micrometeorite impacts on mirror panels significantly affect the optical system performance/life and if there are differences between the various candidate materials in this respect.
- (5) Carry out trades to determine the respective advantages and disadvantages of totally passive vs moderately active mirror panels. For example, it may be possible to use a lightweight and inexpensive material with, say, only focus control instead of another heavier, but passive, segment made of a more expensive material.

B. STRUCTURES AND DEPLOYMENT DEVELOPMENT

Structural concepts for space deployment or assembly of the LDR are required for the primary and secondary reflectors and their support structures. An attractive combination of these two major elements is a great challenge and the combination must be low cost and light weight in addition to being stiff and reliable.

The rationale is that the structural weight has a major impact on the total system; high structural performance and inherent operational reliability and predictability result in a low-cost system. It appears that EVA and remote manipulation will be needed, but the total assembly times available will be short and bounded.

The design trade studies required to optimize the structural system and the generation of a realistic estimate of on-orbit performance can be accommodated only by an analytical process with the capability of accounting for micrometer-level dynamic response. This process will have to accurately account for the effects of structural joint nonlinearity; the identification,

characterization, and simulation of structural damping; and the extension of current capability for simulating structural dynamic behavior to the accuracy needed for the LDR.

Such an analytical capability will significantly enhance the design process used to eliminate or minimize the effects of joint nonlinearities and effectively use the inherent system damping to reduce the amplitude responses and settling time. Additionally, this capability is needed to project how well the LDR structure meets its functional requirements. This technology may reduce the need for expensive flight experiments to characterize the LDR structural systems and validate performance estimates.

A significant portion of the development effort could be for the hardware to be used for the structural characterizations. If hardware for structural joints and full-scale structural models is developed to support other related technology developments and is available for this technology development, substantial resources might be saved.

The development approach should evaluate a number of options for joints and deployable and erectable concepts analytically and with models. Then a baseline selection should be made and developed to the point of performance demonstration with realistic hardware models.

A structural flight experiment may be required to validate the high-fidelity modeling necessary to accurately predict the micrometer-level dynamic responses to achieve the desired LDR performance goals. This experiment would determine the overall dynamic structural behavior of the joints, structural members, panels, and damping in the space environment. In addition, this experiment would validate the construction procedures and the overall structure performance capability. This data base would reduce the program risks and uncertainties associated with high-fidelity modeling capability.

The flight demonstration experiment suggested in Nishioka (1986) is based on a reduced-size LDR system structure that requires the same Space Station services as the full-scale LDR and demonstrates the same levels of performance for the critical technologies.

C. INSTRUMENT DEVELOPMENT

From the results of the SCG analysis (Phillips and Watson, 1984) the items in Table 4-1 can be identified as requiring development in order to be in a position to construct a focal plane for the LDR with "in hand" technology components.

D. PRECURSORS

The LDR will be the premiere telescope built to study the sky in the wavelength region from 30 to 1000 μm . It is appropriate to ask whether the scientific and technological communities are prepared for this task or whether space missions of a smaller scale are required to justify and accomplish the aims of the LDR.

A project of the scope and expense of the LDR relies on a network of proven scientists and engineers and on a solid grounding of mature scientific understanding and technology. For a project costing \$1 to 2 billion, the scientific questions must be crucial and the risk of failure must be minimal.

1. Precursors of Other Premiere Instruments

It is instructive to consider the LDR and its possible precursors in the context of other astronomical telescopes, both space-borne and Earth-bound, at a variety of wavelengths.

a. Optical/UV. In its ultraviolet capabilities the Hubble Space Telescope (HST) was preceded by the Copernicus and International Ultraviolet Explorer (IUE) satellites, while at visible wavelengths the HST represents the acme of many decades of progress in ground-based observations. The HST is, if anything, overdue. The technology is in place and the science more than ready for solutions the HST has to offer. An active and broadly based community of astronomers from around the world will be ready work with the data returned by the HST.

Table 4-1. LDR Instrument Development Requirements

Instrument	Item	Current Status
High-Resolution Heterodyne Receivers		
Detector elements	SIS lead alloy and niobium alloy tunnel junctions	Lead--partially developed niobium--research only
	SIS mixer mounts	Low frequency only
	Photoconductive mixer elements (Ge:Ga, Ge BIB, InSb)	Ge:Ga--partially developed Ge BIB--research only InSb--partially developed
	Schottky diodes (>500 GHz) Schottky mixer mounts (>500 GHz)	Improvement needed in design and T_R
Local oscillators	Solid-state fundamental oscillators	<100 GHz OK >100 GHz research needed
	Schottky varactor diodes (>1000 GHz) Schottky diode multipliers (>1000 GHz)	Improvements needed in design and efficiency
	Molecular lasers	OK, need miniaturization and space qualification
	Laser sideband generators	Partially developed
	High-frequency carcinotrons	Need miniaturization, improved lifetime
Electronics	GaAs FET preamps (1-10 GHz)	1-2 GHz OK, need larger bandwidth
	HEMT FET preamps	Research only
	Back-end spectrometer (digital or AOS)	Need miniaturization and space qualification
Medium-Resolution Spectrometers		
	Large-area integrated photodetector arrays and readouts	SIRTF development, reoptimize for the LDR

Table 4-1 (Continued)

Instrument	Item	Current Status
	Fabry-Perot drives and mirrors Grating positioners	Ground-based and airborne development, some SIRTf development, evaluate for the LDR
Imagers/Imaging Heterodyne Receivers		
Detector arrays	Integrated SIS arrays	Research only, very high priority for development
	Large-area integrated photodetector arrays	OK, SIRTf development reoptimization needed for the LDR
	Bolometer elements	Optimization for LDR needed
Local oscillators	Solid-state oscillators	<100 GHz OK, >100-GHz research needed
	Multipliers	Improvements needed to increase power output and extend frequency range
Preamps	GaAs FET preamps	Research needed for low power array
	Monolithic integrating preamps	SIRTf development, reoptimization needed for the LDR
Special cryogenics	Liquid ^3He refrigerator	OK, space qualification needed
	Adiabatic demagnetization or ^3He - ^4He dilution refrigerator	Research OK, development for space flight needed
Filters	Tunable Fabry-Perot	OK for ground-based and airborne, need study for the LDR
	Long wavelength interference filters	OK for airborne, test for the LDR
Generic Cryogenics		
	Liquid helium	IRAS and SIRTf development, LDR backup approach

Table 4-1 (Continued)

Instrument	Item	Current Status
	Mechanical, closed cycle and J-T stage	OK in lab, need space development, improved efficiency
	Cascaded J-Ts	Research only
	Adsorption refrigeration	Research only
Generic Electronics		
Back-end spectrometers	Digital autocorrelator	OK for ground-based, need study for the LDR
	AOS	OK for ground-based, need miniaturization and space qualification

b. Infrared. The technological and scientific maturity of astronomy at thermal infrared wavelengths was proven by the success of the Explorer-class Infrared Astronomical Satellite (IRAS) mission and is attested to by NASA's willingness to proceed with the Space Infrared Telescope Facility (SIRTF). While SIRTf faces many challenges in the coming years, these will not be due to a lack of urgent science or a lack of viable technological solutions to engineering problems. Infrared astronomy has grown from a relatively small field consisting of a few pioneers observing with ground-based telescopes or making rocket flights to a large field with many capable scientists and technologists working on projects ranging from SIRTf, the Cosmic Background Explorer (COBE), the Spacelab 2 infrared telescope, the NASA Kuiper Airborne Observatory (KAO), and a multitude of ground-based facilities. SIRTf will be the premiere observatory for this community and some of its lessons will be directly transferable to the LDR.

c. High-Energy. In a pattern of growth that is similar in many ways to that of the space infrared community, X-ray astronomers progressed from small rocket flights (Uhuru) through a block of Explorer-class satellites

(Einstein Observatory) up to the current planning for their premiere observatory, the Advanced X-ray Astronomical Facility (AXAF). Einstein proved the technological maturity of large X-ray telescopes and raised questions of critical importance.

The Gamma Ray Observatory (GRO) now under construction has also had its precursors. The SAS-2 satellite and, in Europe, the COS-B satellite have demonstrated the rich character of the diffuse galactic radiation, believed to be a result of cosmic-ray interactions with the interstellar medium.

d. Radio. Radio astronomers have traditionally eschewed involvement in space astronomy, having no need for elevation above an obscuring atmosphere. However, the progress in interferometry leading from the Green-bank 3-element interferometer, the Cambridge 5-km array, and the Westerbork array to the current premiere instrument of centimetric astronomy, the VLA, is very instructive. Many of the technological problems were solved, and many of the scientific questions were answered and reformulated in the course of working with the smaller systems in preparation for building and operating the VLA.

The radio astronomers offer another lesson in the development of Very Long Baseline Interferometry (VLBI). Starting with small networks of antennas, the VLBI community has brought itself to a state of readiness to proceed with a dedicated instrument, the Very Long Baseline Array (VLBA).

e. Millimeter/Submillimeter. We now come to the science and technology of the LDR. Over the last decade a large number of telescopes and receiver systems have become operational at 1-2 mm with excellent performance. The recent progress in making small arrays of antennas operate at these wavelengths (Berkeley and Caltech) has pushed both the science and technology in ways that will be directly relevant to the LDR.

Below 1 mm and particularly below 0.5 mm, however, the number of operating receivers, telescopes, and astronomers drops drastically. The submillimeter community is a small but active one working from the KAO and ground-based telescopes. With the advent of a number of 10-15 m telescopes at dry sites

(notably Mauna Kea in Hawaii), the field promises to grow significantly larger and more mature in both its technical acumen and scientific insight. However, with the exception of the lessons it can draw from the thermal infrared and Earth-resources communities, millimeter/submillimeter astronomers have little experience with the space environment.

2. The State of Technological Readiness for the LDR

The LDR is a most ambitious project; it consists of a 20-m dish that approaches the Hale 5-m telescope in its requirements of surface accuracy expressed in terms of aperture divided by rms tolerance; a structure sufficiently lightweight that it can be launched into space, yet sufficiently stiff and predictable that it can be deployed and aligned virtually automatically; a telescope that must be pointed to subarcseconds of accuracy; a group of focal-plane instruments that push the state of the art in virtually every area; refrigeration requirements that require either thousands of liters of liquid helium annually or coolers that even exist only in the laboratory.

Any of these would be tough problems to solve even if the telescope were fixed to the ground. NASA will have to expend considerable effort resolving these and other questions. Space verification may be required in a few cases.

3. The State of Scientific Readiness for the LDR

The submillimeter community needs to enlarge its base before it will convince the rest of the community to commit the majority of NASA's astrophysics resources for a decade. Congress and the nation have been told that the HST will answer astronomical questions that are absolutely fundamental to our understanding of the universe. There is a similar vital set of questions that demand the LDR to answer them. We must convince our colleagues that the LDR will also do a large amount of fundamental astronomy in a large variety of fields.

A precursor mission is necessary since (1) the new generation of Earth-bound instruments (the Mauna Kea telescopes, the KAO, the SOFIA, and the 3-m balloon telescope) is limited in performance by the atmosphere so badly in the

30-300 μm band that we will be unable to fully define LDR science, and
(2) there are crucial technologies that must be space qualified.

As for the LDR telescope and its structure, a NASA-funded precursor will be of use to verify low weight panel technology. Also, there is a need to solve our particular focal-plane problems. Thus, a science mission designed, as well, to space qualify an SIS mixer, a bolometer array, or a reliable closed-cycle 4-K refrigerator would be very appropriate.

The goal of a precursor mission should be close to the eventual aims of the LDR. Since high spectral and high spatial resolution at high frequency will be the major points to the LDR, it seems logical that as large a telescope as possible operating in the submillimeter range is very important. Using proto-LDR hardware such as cryogenics and advanced mixers is vital.

4. The Scale of the LDR Precursor

The size of precursor missions depends on our assessment of the technological and scientific readiness of the community for the scale of the LDR program that we recommend. The more ambitious the LDR and the farther away we feel we are from being able to achieve it, the more ambitious the precursor must be.

The smallest LDR precursor is a simple dedicated mission such as Spartan. The goal of observing the O_2 and H_2O spectral lines with a 1-m telescope is the model of a small program involving a few astronomers and engineers in a program likely to return useful, but limited, scientific results. This could be extended to have one flight per 1-2 years for, say, 5 years, using a variety of prototype LDR instruments. Such an extended instrument would involve a larger user community and would provide an important technology testbed for focal-plane instruments.

A 3-m telescope in a 747 (SOFIA) may represent a suitable scientific precursor and has many desirable characteristics: frequent reflight capability, significant gain in sensitivity, and 3+ gain in angular resolution compared to the KAO. It will involve, on a regular basis, a growing community

of instrumentation scientists and customers to address essential line and continuum problems. It has for these reasons been given strong endorsement by the astronomical community. The SCG concurs but notes that SOFIA lacks one critical element of an LDR precursor: the driving pressure of preparing submillimeter technology for space that a space project provides.

Perhaps the most ambitious precursor mission is an Explorer experiment. This mission would be to the LDR what IRAS is to the future infrared space missions. A properly chosen Explorer science team could involve several of the major submillimeter centers and give people at all of these places experience in a space mission. This mission could also be an effective testbed for LDR technology and could train a significant and crucial cadre for the LDR.

E. CONCLUSIONS

- (1) Optical, infrared, and X-ray astronomers have built up more of a technological and scientific base, in terms of space experience, than the millimeter/submillimeter community currently possesses before committing to its "premiere instrument."
- (2) A subset of LDR technology problems can and must be addressed by the millimeter/submillimeter community since nobody else will. These include the areas of low noise receivers, detectors, and back ends, and to a certain extent, cryogenic systems and lightweight panels.
- (3) The scale of an LDR precursor is proportional to the scope of the LDR and inversely proportional to our ability to realize the LDR.
- (4) The possible types of LDR precursor range from a \$10 million Spartan to a \$100 million Explorer. While each has advantages and disadvantages, it is essential to develop a broad base of support among the astronomical community for the precursor and for the LDR itself. For if the LDR is to be built, it will be because all astronomers, not just the submillimeter community, feel it is crucial to the continued progress of the science.

- (5) Most important, the millimeter/submillimeter community must clarify and develop the goals of the LDR to make it an urgent and necessary next step for the entire astronomical community.

F. THEORETICAL MODELING

Part of the activities leading up to the LDR must include continued progress in our theoretical understanding of the phenomena to be studied by the LDR. There are two main goals to these LDR precursor activities. The first is to provide us with a better understanding of the predicted line spectra of interesting phenomena so that we can ensure heterodyne coverage at these wavelengths and so that we can better specify the required sensitivity for line detection. The second is to provide us with a better understanding of the predicted morphology and continuum emission intensity of interesting phenomena so that we can ensure that the LDR will operate at the appropriate wavelength, with sufficient angular resolution, and with the required sensitivity. This last issue may educate us to the chopping, nodding, and sidelobe requirements for the LDR. The topics outlined below are candidates for further theoretical studies:

1. Star Formation

- (a) The spatial distribution of the spectrum of disks around proto-stars including the accretion shock.
- (b) Triggers of star formation
 - (1) External triggers
 - (2) Internal triggers and feedback
 - (3) Efficiency and location of star formation. What determines the IMF?

2. Galaxies

- (a) The spatial and spectral distribution of the submillimeter and infrared emission from spiral galaxies.
- (b) Interacting galaxies and the nature of the IRAS "infrared galaxies."
- (c) The deep sky in the infrared. What do distant galaxies look like at 1-4 μm and at 100 μm ? What would high-angular-resolution pictures show us?

3. Cosmology

Theoretical models of the small-scale (~ 1 arcsec-1 arcmin) anisotropy of the cosmic microwave background.

4. Fundamental Parameters

Quantum mechanical calculations of collisional cross sections for rotational excitation of H_2O by H_2 .

G. LABORATORY ASTROPHYSICS

- 1. Wavelengths of Key Astrophysical Transition (e.g., NII fine structure)
- 2. Collision Cross Sections of Key Transitions (e.g., rotational excitation of H_2O by H_2)
- 3. Laboratory Studies of Interstellar Dust
 - (a) The spectra and optical constants of various materials

(b) Photochemical and photophysical studies of ices

(c) Formation (nucleation) and destruction of grains

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APPENDIX A
FURTHER SCIENCE JUSTIFICATION ARGUMENTS

1. GALAXIES AT HIGH REDSHIFT

The LDR offers the possibility of studying the early evolution of spiral galaxies at redshifts greater than $z = 1$. The far-infrared continuum in spirals results from starlight from young stars that has been absorbed in the visual and ultraviolet and then reemitted by dust at long wavelengths. Far-infrared measurements can be used to study the evolution of the star formation process as a function of look-back time. High-spectral-resolution measurements in the CII line at a rest wavelength of $158 \mu\text{m}$ can be used to determine the masses of distant spiral galaxies and by using the Fisher-Tully relationship to measure the geometrical properties of the universe itself. However, to accomplish these goals the spatial resolution of a 20-m LDR is required.

Consider a cluster of galaxies at a redshift of unity where the typical galaxy size is 1 arcsec and the typical galaxy-galaxy separation is 20 arcsec. To make an unconfused measurement of an individual object requires a minimum separation between objects of about 5 beam widths. To measure a galaxy at its far-infrared peak of $100 \mu\text{m}$ (restframe) requires a measurement at $200 \mu\text{m}$ where the beam size (FWHM) of the 10-m LDR is 6 arcsec and the 20-m LDR is 3 arcsec. The smaller LDR will be confusion-limited by cluster galaxies themselves and be unable to make the necessary observations.

The strongest spectral line from the warm interstellar gas found in spiral galaxies is probably the $158\text{-}\mu\text{m}$ transition of single ionized carbon, CII. Our own galaxy emits 0.1 percent of its entire luminosity in this transition (Stacey et al., 1985). The $158\text{-}\mu\text{m}$ line can be used to characterize the thermodynamics of the interstellar medium and to give the total mass of the galaxy. Using the LDR, this line can be used to determine the properties of spiral galaxies out to the edge of the known universe. At $z = 1$ a galaxy like our own could be measured with a signal-to-noise ratio of 5 in an hour's integration with 30 km/s spectral resolution using a 20-m LDR. Because of confusion with other cluster members, the small beam size of the 20-m LDR, 2 arcsec at $300 \mu\text{m}$, is crucial for this program. Once calibrated by the study of relatively nearby galaxies, the CII line can be used to measure the metric of the universe itself using the Fisher-Tully relationship between the width of the CII line and the total luminosity of the galaxy.

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These exciting scientific prospects, among others, require the sensitivity and the angular resolution of the 20-m LDR. The search for extra-solar planets pushes the wavelength of shortest operation to 30 μm while demanding an exquisitely precise beam shape. The extragalactic projects demand a 6 arcsec beam size at 300 μm to avoid confusion effects from cluster and background galaxies.

2. EXTRA-SOLAR PLANETS

One project of crucial scientific importance that goes from being marginally possible with a 10-m LDR to becoming palpably real with a 20-m LDR is the direct detection of extra-solar planets. To date, no clear evidence exists for the existence of planets orbiting stars other than our sun. While IRAS has detected shells of dust and debris orbiting nearby stars such as Vega and Beta Pictoris, IRAS lacked the sensitivity and resolution to detect the thermal emission from a single-like planet. The detection of planets around a statistically significant number of stars would provide quantitative data to test theories of planetary formation. A deep understanding could be obtained of how many planets exist in the entire galaxy, a number of great significance in the search for extraterrestrial life.

How would the LDR be used to detect other planetary systems? The detection of thermal emission from a planet at two wavelengths yields the temperature of the planet. From its temperature and overall energy output, its size can be determined. Once the orbit of the planet has been determined, one can deduce some of the planet's internal properties such as its constitution and energy balance.

The detection of planets drives the LDR toward the largest apertures and the most precise figure at the shortest possible wavelengths. The distance to which a 100-K, Jupiter-sized planet can be detected in a 3-h integration at 30 μm , the wavelength of peak emission, is approximately $2(D/10 \text{ m}) \text{ pc}$, where D is the diameter of the LDR. There are two stars within 2 pc, but 26 within 4 pc. Thus, a 20-m LDR is required to search a meaningful sample of stars. Assuming that we have a 20-m LDR with diffraction-limited resolution at 30 μm and assuming that the sidelobes at 3-10 arcsec are 30 dB below the peak

response, the LDR will be able to spatially resolve Jupiters that lie within 10 AU of their stellar primary. Degrading the sensitivity or angular resolution of the LDR by relaxing the size, the minimum wavelength of operation, or the sidelobe requirements would make it impossible to detect a planetary system like our own around another star.

3. OBSERVATIONS OF EVOLVED STARS

At the end of their main-sequence lifetimes, stars develop extended envelopes which are loosely bound and stellar winds which can reach truly staggering rates (up to $10^{-4} M_{\odot}/\text{yr}$). In many cases the layers being expelled have been enhanced in heavy elements which have been produced in the stellar cores, as evidenced by the incidence of carbon-rich objects among the intermediate-mass stars ascending the asymptotic giant branch. The evolution of such stars is very poorly understood for several reasons including (1) the uncertain role of mass-loss in further evolution, (2) the importance of mixing in changing opacity laws, and (3) thermal instabilities associated with single- and double-shell burning. Red giants are often cited as major producers of several of the most abundant heavy nuclides, yet little is known about this activity because the heavily shrouded stars primarily responsible for galactic enrichment are not readily observable at optical wavelengths.

The LDR is the ideal telescope for study of dense winds from evolved stars. The circumstellar shells are invariably small. Ground-based, filled-aperture telescopes at millimeter wavelengths lack the needed gain (antenna beam width) to observe more than a handful of stars, and even from currently available observations it is apparent that we are dealing with a very heterogeneous set of objects. Ground-based interferometry will not help much because of the low surface brightness of such shells. A couple of simple calculations make the need for the LDR apparent.

Consider a star with $dM/dt = 10^{-5} M_{\odot}/\text{yr}$. Assuming that a molecule is observable out to a radius where $n = 10^4$, this implies a radius of 0.02 pc, necessarily a short one ($< 10^5$ yr at such a rate) so that the distance needed to obtain a suitable number of stars for an LDR survey is about 500 pc. At that distance, the size of the envelope is 6 arcsec, equal to the LDR beam

width at 300 μm . For most molecules, infrared pumping is more important than collisional pumping and the apparent envelope size is even smaller. The mass-loss mechanism is poorly understood, and mapping the circumstellar material is essential to determine the possible roles of equatorial winds (expulsion into a disk), the roles of binary companions (as suggested by the occurrence of bipolar nebulas), episodic mass-loss events (as suggested by line-of-sight infrared observations), and the importance of large convection cells and/or large-scale magnetic phenomena (which might give rise to disordered structure).

An especially important role which might be served by the LDR is to allow for a survey of circumstellar shells in the Magellanic clouds. A grave uncertainty in the study of evolved stars is a knowledge of their distance. They are usually field stars and their intrinsic luminosities are not known. In the Magellanic clouds, mass-loss objects would be readily observable at the shortest LDR wavelengths so that a systematic survey could be made which could associate mass-loss rate and abundance type with luminosity. Assuming the same parameters as above, and an observing wavelength of 30 μm , the LDR beam dilution would be about a factor of 100. Assuming a surface brightness of 20 K, which would imply an antenna temperature of 0.2 K, would allow for a survey at 30 μm . Again, it is the enormous forward gain which would make such observations possible.

4. OBSERVATIONS OF SOLID BODIES IN THE OUTER SOLAR SYSTEM

The objective here is to determine the albedo and sizes of solid bodies in the solar system. This includes asteroids, satellites, and cometary nuclei.

With broadband visible and thermal infrared data, the size and albedo of solid bodies can be estimated. This has already been very successfully applied to asteroids. With the LDR we can extend this work to the outer solar system. We want to use this data to better understand the history of asteroidal fragmentation and the relationship of the solid bodies in the outer solar system to those in the inner solar system.

C-2

The large aperture of the LDR is required to study very small bodies (<10 km) to as large a heliocentric distance as possible. Both large aperture and far-infrared capability is required.

5. STUDIES OF STRATOSPHERIC EMISSION LINES FROM PLANETARY ATMOSPHERES

The objective of this study is to determine the composition of planetary stratospheres in order to study the photochemistry. The far-infrared is important since many gasses have strong bands at these wavelengths. For the outer planets, these studies are important because we can study complex hydrocarbon photochemistry and search for biologically interesting molecules.

The LDR is needed because the velocity resolution required is similar to that in the ISM: Doppler-broadened at 170 K for Jupiter and Saturn, 100 K for Neptune. Therefore, high spectral resolution is required at far-infrared wavelengths, and the large aperture of the LDR is needed for high sensitivity. We also need the high spatial resolution of the LDR in order to study latitudinal effects on spatially resolved planets, such as Venus, Mars, Jupiter, and Saturn.

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APPENDIX B
TYPICAL OBSERVATIONS

1. STRATEGIES AND TOPICS

For many observations the LDR's capability depends entirely on having detector arrays, or even heterodyne mixer arrays, in the focal plane. This can be seen by comparing the time required to map 1 square arcmin (a small region for most studies) in different configurations. The following paragraphs describe a few typical observations that will be routine for the LDR in studying many objects. There is a tremendous difference between the time required for a single detector vs that required by focal-plane arrays of 100 or 100 x 100 detectors. Detector arrays working at wavelengths of 30-300 μm are clearly necessary, enabling technology for many of LDR's scientific goals.

2. STAR FORMATION REGIONS

a. Continuum Mapping

Mapping dust clouds with low spectral resolution ($R = 30$) will give the distribution of the optical depth, which shows the dust concentration. This will be done by SIRTf at short wavelengths with low angular resolution, but for high resolution the LDR is needed at 30 μm , and to make maps, the LDR is needed at 300 μm where SIRTf's sensitivity and resolution are poor. For mapping cold clouds, only the LDR will be useful because cloud emission is too weak below 300 μm . Two extreme cases are (1) mapping a warm cloud ($T_{\text{kin}} = 50 \text{ K}$) at 30 μm with high angular resolution (3 arcsec) and (2) mapping a cold cloud ($T_{\text{kin}} = 50 \text{ K}$) at 300 μm . In each case we want to be able to detect brightness variations of 10^{-3} or 2×10^{-4} for an optically thin source).

For the warm cloud this sensitivity requires an integration time of 45 s per point, so in raster mapping mode a single detector would require 500 hours of integration to map 1 square arcmin, or about 1500 periods assuming pointing and drive efficiency of 80 percent. For a focal-plane array of 100 x 100 detectors, 1 square arcmin could be mapped in about 4 min. For a linear array of 100 detectors, 1 square arcmin could be mapped by two driven scans of 150 min each (drive rate 0.4 arcsec per min; note that the scans would have to

be interrupted and restarted many times). Mapping this warm cloud at 100 or 300 μm is much easier because the emission is so much stronger and the beam is larger so we need fewer points to fully sample the area. At 300 μm this sensitivity is achieved in less than 10^{-3} s. Mapping 1 square arcmin with a linear array of 100 detectors at 300 μm would require less than 1 s driving as fast as possible.

The cold cloud ($T_{\text{kin}} = 10$ K) could be mapped with this sensitivity (2×10^{-4}) only at 300 μm (at 100 μm this would need 30 years of integration per point, at 300 μm it takes only 1 s per point). In a raster mapping mode with a single detector this takes about 8 min per square arcmin; with a 100 x 100 detector array it takes only an average of 0.04 s per square arcmin; with a linear array of 100 detectors it takes an average of 4 s per square arcmin (driving at the sidereal rate = 15 arcmin per minute).

b. Velocity Tracing Using Strong Lines

Typical lines which will be useful as velocity tracers in dense dust clouds may be 63- μm OI and CI at 370 and 610 μm . Assume that some line will have optical depth of about 0.1 over a large area of the cloud with a velocity width of 3 to 5 km/s. To be useful for detailed tracing of the velocity field (e.g., for fragmentation process studies) the observations should be done with high spectral resolution, $R = 10^6$ (0.3 km/s). Assuming typical cloud temperatures of 50 K then at 63 μm it would take an integration time of 10 min per point to measure the profile of such a line (5 σ in each of 10 velocity channels). At this wavelength the beam size is 0.6 arcsec, so mapping a square arcmin would take a single receiver about 2 months of integration. Using the one-dimensional array would take about 17 hours per square arcmin (41 periods), driving at 0.5 arcsec per min, while the 100 x 100 array would need only 10 min per square arcmin. Clouds much colder than 50 K will not be easy to map at 63 μm , even if the line is optically thick. At 370 μm a 10-K cloud with an optical depth of 0.1 would need about 12-min integration per point, at 610 μm only 50-s integration. Since the beam is large (4 to 6 arcsec) the mapping rate is fairly fast. Molecular lines like the CO ladder will be useful for this as well, but this begins to overlap ground-based, millimeter-wave interferometers and submillimeter single dish work.

c. Chemistry

When observed with moderately high spectral resolution ($R = 10^5$) many molecules with lines in the 30–300 μm range will be of interest for studies of interstellar chemistry or element abundances. To detect such a line with an optical depth of 10^{-4} in a warm, dense molecular cloud core ($T = 50$ K) at a wavelength of 300 μm would take about 20 hours (50 periods), assuming the emission region filled the beam. At 30 μm , the weakest line detectable in 20 hours would have an optical depth of 0.3 at $T_{\text{kin}} = 50$ K, 3×10^{-5} at $T_{\text{kin}} = 500$ K. Multiple detectors will help only if the emission region is extended much larger than the beam, which may be true at 30 μm (0.3-arcsec beam) but might not be at 300 μm (3-arcsec beam, cf. Orion BN).

d. Interstellar Thermodynamics

A crucial tracer of the ISM thermal history, ionization, and radiation field is the CII line at 158 μm . To date this has been detected from extended CII zones around HII regions, from active spiral galaxies, from the edges of molecular clouds, and perhaps from the general diffuse ISM. Typical intensities in the galactic plane are $10^{-3} \text{ erg cm}^{-2} \text{ s}^{-1} \text{ sr}^{-1}$ (Stacey et al., 1985); assuming a line width of 30 km/s this flux will be 11 Jy/beam for the LDR. Mapping this line takes so little time per point (7×10^{-6} s for a detector array with $R = 10^4$) that a survey of the entire galactic plane at full resolution (1.6 arcsec) would be feasible in a short time. To cover 10^3 square deg ($-1.5^\circ < b < +1.5^\circ$) would take a linear array of 100 detectors about 70 scans around $0^\circ < l < 360^\circ$. Assuming the maximum drive rate with accurate pointing is 5 deg per min, this entire survey would take about 85 hours (assuming the perfect inclination between the LDR orbit and the galactic plane so the scans need not be interrupted to avoid the Earth, sun, or moon). This drive rate of 5 deg per min (5×10^{-3} s per beam width at 158 μm) gives a signal-to-noise ratio on the line of 260 at $R = 10^4$. This operation mode puts a heavy burden on the data transmission rate; assuming 100 rows of detectors each with 100 spectral channels for which 16 bits must be read out every 5×10^{-3} s the data rate is 30 Mbit/s^{-1} . (Mapping at this rate with a 100×100 heterodyne array with spectrometers having 256

channels on each mixer at a wavelength 30 μm would require 4 Gbaud! Even without sending the mixer intermediate frequency (IF) down, the LDR needs large communications bandwidths.)

Finally, to map the 158- μm line with very high spectral resolution ($R = 10^6$) and high sensitivity in selected small areas, e.g., compact II regions would take about 5 s per point. A single element would thus need 30 min to cover 1 square arcmin, an array of 100 x 100 mixers could cover about 7 square arcmin per second if the telescope could be positioned that fast. A linear array of 100 detectors could cover 0.8 square arcmin per minute, driving at a rate of 0.3 arcmin per minute.

The upshot of these calculations is that the LDR is extremely powerful for a wide range of scientific studies. Integration times are relatively short for many observations. But to map a reasonably large area (a few square arcminutes minimum) will be prohibitively slow for a single detector or heterodyne mixer. The focal plane MUST be multiplexed by an array of detectors to make mapping feasible in most cases. We recommend an ambitious development program for these detector and heterodyne arrays.

3. REFERENCE

Stacey, G. J., Viscuso, P. J., Fuller, C. E., and Kurtz, N. T. (1985), Ap. J., 289, 803.

APPENDIX C

THEORY OF OFFSETS CAUSED BY RANDOM DEFECTS AND DUST

Any chopping system can cause the beam "illuminated" by a fixed detector to move both on the sky, which is desired, and on optical surfaces, which is not desired. The area of a mirror with changing illumination, ΔA , typically consists of two crescent-moon-shaped sections around the central area A of the mirror. Dust particles and defects within ΔA cause background changes modulated by the chopper and lead to a random component of the offset. If one considers a single dust grain with radius a , and with optical efficiency factor $Q(a, \lambda)$, the dust grain will radiate a flux given by $\pi a^2 Q B_\lambda(T_m)$ where T_m is the temperature of the mirror and the dust grains on it. The area $\pi a^2 Q$ is changed from intensity given by the overall system limit $\epsilon B_\lambda(T)$ to this higher value, so the change in background is

$$\Delta B = \frac{\pi a^2 Q}{A} \cdot \frac{B_\lambda(T_m)}{\epsilon B_\lambda(T)} \cdot B_{\max}$$

If the dust density on the surface is $n(a)da$ then the number of grains within the area ΔA is $\# = \Delta A \int n(a)da$, and for randomly distributed grains the variance of the number = $\#$ as well. When integrated over a grain size distribution, and summed over different mirrors, the result is

$$\sigma \left(\frac{\Delta B}{B_{\max}} \right) = \frac{\pi}{E} \left\{ \sum \frac{\Delta A}{A^2} \left[\frac{B_\lambda(T_m)}{B_\lambda(T)} \right]^2 \int a^4 Q^2 n(a) da \right\}^{1/2}$$

Various types of choppers achieve different values for $\sum \Delta A/A^2$ which can be used as figure of demerit for choppers. The general rule for a good chopper is to chop as close to the primary as possible and to not allow the beam to move on a surface that is close to an image of the sky.

1. CHOPPING PRIMARY

While not a practical chopper for large telescopes, this chopping design gives $\Delta A/A^2 = 0$.

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2. CHOPPING SECONDARY

For a 20-m LDR with a $F/D = 10$, a 1-arcmin throw causes a displacement of 60 mm in the focal plane. For a chopping secondary the displacement on the primary is also 60 mm, so the area of each crescent is 1.2 m^2 . Since there are two such crescents the value of $\Delta A/A^2$ is 0.00002 m^{-2} . Working through the rest of the numbers, a surface with 15 to 25 percent of its area covered by 100- μm radius dust grains could meet the $\Delta B/B = 10^{-5}$ specification. Large irregularities are much more critical: a single 1-cm diameter black flaw in the unbalanced area would be very serious. The area of the cracks within the unbalanced area must be stable and balanced, or the cracks must be given an emissivity equal to the mirror. However, a chopping secondary system should easily achieve the specification without requiring very clean mirrors.

3. FOCAL-PLANE CHOPPERS

A focal-plane chopper working 1 m from the focus has a beam area of 0.008 m^2 . For a rotating mirrored fan blade chopper, $\Delta A/A^2 = 2$ since the two beams see different mirrors. Thus, the figure of demerit is 260, about 10^7 times worse than the chopping secondary. A push-pull diagonal focal-plane chopper gives $\Delta A/A^2 = 0.12$ for a 1-arcmin throw, so it is 20 times better than the rotating mirror chopper. Focal-plane choppers can be improved by moving farther from the focus, but they will always be very poor choppers compared to a chopping secondary. The beam will also move on either the secondary or the primary, but the beam area on these mirrors is so much greater than the beam area at the chopper that the contribution of the big mirror to $\sum \Delta A/A^2$ is negligible.

4. JPL CHOPPING QUATERNARY

Compared to a Cassegrain chopping secondary LDR, for which the beam moves only on the primary, the JPL chopping quaternary has the beam moving on two surfaces: the secondary has $(\Delta A/A^2)$ 858 times greater than $(\Delta A/A^2)$ for the primary of the Cassegrain system, while the tertiary has $(\Delta A/A^2)$ 9678 times greater than the primary of the Cassegrain system. Thus, the secondary of the JPL design should be at least 2000 times cleaner, and the tertiary at least

20,000 times cleaner than the primary of a Cassegrain system. In the usual terms of surface cleanliness class, one finds that the a^4 factor in the integral over $n(a)$ means that the largest particles dominate the integral. Thus, the statement that the JPL chopping quaternary system requires 20,000 times cleaner mirrors translates into a largest particle that is about 12 times smaller. In fact, the chopping secondary design can tolerate a Class 10,000 surface on the primary, while the JPL design requires a Class 500 surface on the tertiary and Class 900 on the secondary. This will be hard to do, but not necessarily impossible.

5. GAP STABILITY IN A SEGMENTED MIRROR

The gap in a segmented primary needs to be very stable in a chopping secondary design, but can vary in the JPL chopping quaternary design. For an LDR mirror made with 2-m segments, the length of the gaps in the area ΔA is about 60 cm. If the gap widths were to increase by $167 \mu\text{m}$ on one side of the primary, $\Delta B = 10^{-5} B_{\text{max}}$ would result. But this ΔB would vary as the gaps "breathed," leading to changes in ΔB that would spoil the overall photometric accuracy. If the breathing period were on the order of 10 s, then achieving the overall photometric accuracy specification of $10^{-8} B_{\text{max}}$ in 10^5 s would limit the breathing to $17 \mu\text{m}$, or about 10 parts per million. Since moving the telescope may well excite modes that cause the gaps to breathe, a systematic photometric error may result, which could lead to more stringent requirements.

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16. Abstract <p>The Large Deployable Reflector (LDR) is a telescope designed to carry out high-angular resolution, high-sensitivity observations at far-infrared and submillimeter wavelengths. NASA has carried out several system studies of the LDR and has held two workshops, in June 1982 and March 1985, at Asilomar, California. A Science Coordination Group (SCG) was established by NASA in September 1983 to review the goals of the LDR, to provide definitive design criteria, and to interact closely with the technical studies. This report of the SCG activities complements the several study reports and the Asilomar workshop reports. Some of the major issues addressed in the SCG report are discussed below.</p> <p>The scientific rationale for the LDR is discussed in light of the recent Infrared Astronomical Satellite (IRAS) and Kuiper Airborne Observatory (KAO) results and the several new ground-based observatories planned for the late 1980s. The importance of high-sensitivity and high-angular-resolution observations from space in the submillimeter region is stressed.</p> <p>The scientific and technical problems of using the LDR in a "light bucket" mode at $\lesssim 5 \mu\text{m}$ and in designing the LDR as an unfilled aperture with subarcsecond resolution are also discussed.</p> <p>The need for an aperture as large as 20 m is established, along with the requirements of beam-shape stability, spatial chopping, thermal control, and surface figure stability. The instrument complement required to cover the wavelength-spectral resolution region of interest to the LDR is defined.</p>			
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